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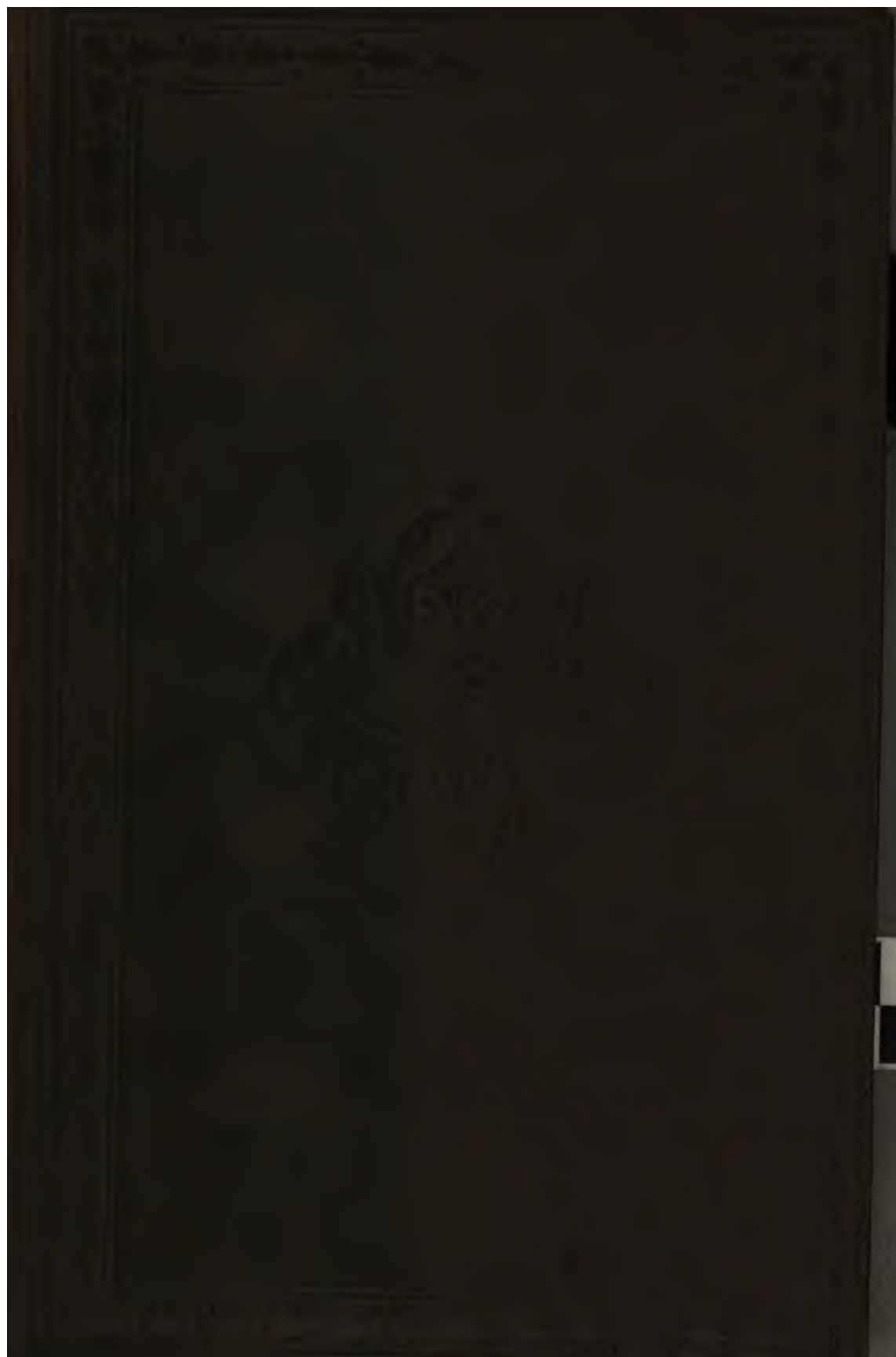
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CLINICAL HISTORIES

WITH

COMMENTS.

BY

HENRY DAY, M.D.,

MEMBER OF THE ROYAL COLLEGE OF PHYSICIANS:
PHYSICIAN TO THE STAFFORD COUNTY INFIRMARY.



LONDON:

JOHN CHURCHILL & SONS, NEW BURLINGTON STREET.

1866.

157. m. 58.

TO
SIR THOMAS WATSON, BART., M.D.,
FELLOW OF THE ROYAL SOCIETY,
PRESIDENT OF THE ROYAL COLLEGE OF PHYSICIANS,
PHYSICIAN EXTRAORDINARY TO HER MAJESTY THE QUEEN.

Sir,

As a Member of the ancient and distinguished medical body of which you are now by universal assent the head, I dedicate this book to you. Further I do so dedicate it, not merely as acknowledging your high office, but also the works that have led you to the office.

I wish moreover to recognise in the dedication your faithfulness to your brethren, your devotedness to your art, your success as a practitioner, and your classical, not less than your practical teachings of Medicine.

I am, Sir,

With sincere respect,

Your friend,

HENRY DAY.

P R E F A C E .

THIS book requires but few remarks in the way of introduction. Whoever remembers the old volumes of "Medical Facts and Observations," will recognise the suggestion on which the book has been framed. I have made every history rest upon an observed fact or facts of disease. I have tried to read those facts by the forward light of my time, and in some cases I have ventured to comment, as a practitioner purely, on points of difficulty or doubt.

I do not profess to claim for a work of this character the highest position in medical research: it is not the highest. It is very homely, primitive I had nearly said, and aims once and for all to be no more than useful. By it I have tried to speak to my brethren in common language on subjects in common, not as presuming to teach them, but as endeavouring to quicken their

interest in special subjects, and to elicit from them what they also think. The plan of the book is thus very simple, and very free from originality; but after all it is on the plan of the earlier steps in the history of our art, and it will probably last so long as the art lasts.

Its disadvantages are that it is diffuse and unsystematic: a language without a grammar and without a shade of poetry. Its advantages are that it enables an observer who feels he cannot build on brilliant originality to build on industry; and, if it does no other service it makes him think, learn, and cast off the rust of mere routine.

Thus I lay my book before the critic, an open book, asking him only to do it this fairness, that he do not criticise it friendlily nor otherwise than as a book of facts and observations in physic.

Stafford, October 2nd, 1866.

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CLINICAL HISTORIES

WITH

COMMENTS.

HISTORY 1.

CEREBRO-SPINAL MENINGITIS.

There must be frequently occurring to almost any actively and thoughtfully employed member of the Faculty of Medicine, cases which, though described generally in "Systems of Medicine," yet perhaps have never been actually *seen* by the writers thereof, so that peculiarities, differences, and anomalies are but slightly, if at all, referred to. It is to such cases, principally but not wholly, that I purpose to direct attention in the following "Clinical Histories," which I offer, not as containing any new or original ideas, but as contributions to the literature and experience of the Profession in a form and manner that may, I hope, prove both interesting and instructive.

In the present condition of our knowledge as regards the functions of the "Nervous Centres," and the affections they are liable to be afflicted with, it cannot, I believe, be asserted, or at any rate success-

fully maintained, that our information is of such a sound and reliable character as to afford anything like real satisfaction. One need only refer to "Epilepsy," and "Chorea," and other spasmodic nervous disorders, to feel the impotency of our knowledge, and to force from us the admission that whatever may now be known as to their etiology and treatment, there is infinitely more still to be learned respecting these maladies.

The very peculiar nervous affection, called "Spotted Fever," which prevailed as an epidemic in Germany with such terrible fatality during last year (1865), I consider to be a phenomenon of no ordinary interest. To me it is peculiarly interesting from the circumstance that I have had the opportunity of observing two cases occurring sporadically in this country. The one took place in the year 1859; the other in the autumn of last year.

I do not think it is desirable, I am sure it cannot be necessary, to enter into any discussion as to the propriety of applying the term "Fever" to this affection, because there is scarcely any acute disorder or disease that is not accompanied by the condition to which the term fever may be applied, but that the generic phrase, if I may so express myself, of "Typhus" or "Typhoid" is ever correctly made use of in the present instance admits of, in my opinion, the gravest doubt. The pathological condition of certain parts made evident

by *post mortem* examination, affords moreover ground for a nomenclature sufficiently descriptive and certain to satisfy the most fastidious: the evidences of inflammation of the meninges of the brain and spinal cord, together with the effusion, supply unmistakeable proof of the mischief that has been going on, and therefore the name "Cerebro-Spinal Meningitis" seems to me to accomplish every requirement. In both my cases the eruption was present during life, yet I do not believe its presence is essential to constitute the disease, whilst the term "Epidemic," applied as a prefix in this country, would be obviously incorrect, for hitherto we have escaped so lamentable an infliction in the shape of an epidemic, although, as is well known, in other countries—Germany and America—the disease has made its visitations after this manner.

A case which I shall relate after a while brings to my mind the one I am now about to give an account of, and I permit this to have the precedence only because its date is anterior. As some time has elapsed since it took place, I shall merely state the bald facts as they appear in my case-book, and reserve any comments until both cases have been described:—

CASE.

Early in the morning of October 17th, 1859, I was requested to see G— G—, residing with his

parents a few miles from Stafford. He was a carpenter, or, as it is called here, a "joiner," by trade, and worked for his father, who was also a "joiner" and had two or three acres of land.

The young man was just turned 17, and my book gives the following as his "*Previous History*:" "He had always been perfectly steady in his habits, but had never had robust health; in fact he had been delicate, although he never had any serious illness. He used to suffer repeatedly from indigestion and from constipated bowels. Both his father and mother were alive, but they had never enjoyed very good health: their circumstances were comfortable, but, being of saving habits, the food the family consumed was not only of the simplest but sometimes was not the most wholesome. Animal food, in the solid form, was not taken more than two days in the week. The youth was attacked soon after supper, which consisted of 'supping,' *i.e.*, a very weak broth and bread, on the night of the 16th of October, with pain in the stomach, vomiting, and involuntary and incessant 'twitchings' of the limbs; he also complained of great dimness of vision."

His state, when I first saw him, was as follows: "He was in bed, and constantly on the move. The limbs, and sometimes with them the body, were jerked spasmodically in all manner of directions. His countenance was flushed, and expressed great anxiety: his speech was not

very distinct, and he 'snapped' out his words. Occasionally he uttered a short scream; his vision was very imperfect, and the pupils were contracted to mere 'pin points;' he did not appear to know at times what was said to him; he complained of pains in the stomach and limbs, but if pinched he did not seem to feel it; the tongue was furred in the centre, the edges looking bright-red—swollen and indented by the teeth: the pulse was 110, but very variable, being sometimes much quicker than at others: it was hard and thin. The respiration was very hurried and laboured, the abdominal muscles appearing to be the principal auxiliary muscles of respiration that were called into play. The urine was high-coloured: and of specific gravity, 1.020: it was very slightly acid, and had a peculiar ammoniacal odour. There was no evidence of albumen, and no sediment. He was constantly vomiting."

Following the order in which the case appears in my book, the next point referred to is "*Diagnosis*," and this is given as "Inflammation of the Membranes of the Brain and Spinal Cord."

The treatment commenced with 24 leeches to the back of the neck. An enema of Ol: Ricini. Sp: Terebinth: and Gruel. At 7 p.m. a blister was applied to nearly the whole length of the spine, and he was ordered to have two grains of opium. At 10 a.m., as the violent spasmodic movements were in no way lessened, the

opium was repeated. A chloroform pad was applied to the epigastrium in the hope of diminishing the sickness, and at 9 p.m. he was put into a warm bath, at the temperature of 96°, and another dose of opium was administered.

“The treatment appeared to produce no satisfactory result whatever:—the enema acted by producing two evacuations—the first somewhat large and solid, the second much thinner, but both perfectly natural in appearance and odour. The first dose of opium seemed to effect a momentary amelioration of the spasms which, however, never altogether ceased from the time he was attacked until his death, which took place at one a.m. on the 19th.”

COMMENT.

My “*Remarks*” made at the time I give verbatim:—“The attack was so sudden and unexpected, and the vomiting so constant, that, as he had recently had a meal, I, in the first instance, thought he might have been poisoned, but careful enquiry soon led me to an opposite conclusion. Others had partaken of the same kind of food, made of the same materials, and cooked in the self-same vessels, and a dog that was in the house at the time ate a portion of the food first vomited and was in no way affected injuriously.

“On the morning of the 18th an eruption made its appearance on the limbs and front of the body

—it was in patches, not like measles in appearance or colour, but more resembled ‘flea bites or the bites made by midges.’ He vomited continually, and about 1 a.m. on the 19th he had a violent spasmodic paroxysm—threw himself on his back—stretched himself out, and died. When I saw him at 9 a.m. his back was slightly arched, his feet very much so, and his thumbs turned into the palms of his hands.”

Post mortem appearances. “All the organs of the thoracic and abdominal cavities appeared healthy; stomach empty; coverings at the base of the brain and those of the spinal cord congested, not uniformly, but in patches; a large amount of fluid at the base of the brain.”

So far, then, for the case as it appears in my book, but as the case was frightful in appearance, and I had not seen anything like it in the course of my practice before, I have little or no difficulty in supplying some additional particulars, which I carefully collected at the time.

I was informed that the young man had some two or three miles to walk to and from his work, and that he frequently got wet and did not change his clothes. He had had no attack of acute rheumatism. I succeeded, but with considerable difficulty in consequence of his incessant and uncontrollable movements, in making a stethoscopic examination of the sounds of his heart, and I was able to say, positively, that no pericardial or endocardial

murmur was heard with either sound, but the rythm seemed constantly altering.

The friends offered considerable opposition to the post mortem examination; and after my inspection of the brain and spinal cord, I was interrupted whilst examining the viscera and obliged to desist, so that these organs were not as carefully examined as I should have wished.

The right side of the heart, I remember, was filled with dark-coloured blood of about the consistence of treacle. The membranes on the upper part of the brain were not so much congested as at the base; the general character of the congestion was "patchy," and the same condition existed in the membranes of the spinal cord—that is to say in patches—some parts seemed almost natural, and then there would be a larger or smaller patch of florid congestion; there did not appear to be an exceedingly large amount of fluid in the spinal canal, but there was an immense quantity at the base of the brain. I was given to understand that the pupils of both eyes were widely dilated at the last.

As I was, on first seeing the patient, on the look out for poison, I remember examining all the food very particularly, and I recollect being struck with the soft pasty look of the bread that was shown to me. I spoke about it, and the mother informed me that the wheat from which the flour had been prepared was not so "sound"—whatever that may mean—"as they usually had it."

CASE THE SECOND.

The next case I had the opportunity of seeing was in the Stafford General Infirmary. It was admitted under the care of my colleague, Dr. Harland, by whose courtesy I am permitted to give the following report:—

George Giltrop, aged 10, was received at the Infirmary as an in-patient on September the 1st, 1865. He had been seen previously to his admission by one of the Honorary Surgeons to the Institution, Mr. Weston, who advised his removal to the Infirmary. Mr. Weston informed me that he had been sent for to see the youth in consequence of his having pain in the ankle, which part upon visiting him, he found to be swollen. He also, however, found him affected with violent spasmodic movements of the extremities, perfectly involuntary, and an eruption both on the body and limbs. He vomited repeatedly, and Mr. Weston came to the conclusion that the Infirmary would be the best place for him.

On admission his limbs and body were observed to be covered with an eruption, in patches, resembling in appearance and colour that described in the previous case. His movements in bed, where he was placed instantly on his arrival, were spasmodically incessant, requiring the constant atten-

tion of a nurse to prevent his being jerked out of bed; his tongue was furred in the centre, the edges being red; his pulse was hard, thin, and quick, the rate of the pulse being more or less rapid just as the spasmodic movements were more or less severe. It could not be determined that he saw at all, the pupillary openings being so contracted as to appear almost obliterated. It was thought that he heard when spoken to, but as he gave no reply to my enquiries and never spoke from the time of his admission, this point is, I think, very doubtful.

I pinched and tickled him, but he showed no signs of feeling either the one or the other. He vomited frequently, and shortly after his admission had some diarrhoea, but this latter soon stopped. His spasmodic movements never ceased, and he died about twelve o'clock on the night of the 2nd.

The nurse who was with him at the time of his death said that he had at the period just named, *i. e.* twelve o'clock, one spasmodic paroxysm more violent than she had seen him have before. He threw himself on his back, resting on the back of his head and his heels, his body being much arched, and then died.

TREATMENT.

When he first arrived at the Infirmary he was seen by the House Surgeon, who learning that he had had a meal shortly before he was attacked, thought it both possible and probable that he had

eaten some poisonous food, and therefore ordered him an emetic, which acted readily, but produced only fluid of an ordinary character. He then prescribed for him a mixture containing sesqui-carbonate of ammonia, sesqui-carbonate of soda, ether, and tincture of camphor. Subsequently a blister was applied to the back of the neck, and when diarrhoea began, an astringent mixture was administered. Not a single improvement however took place in his condition, and he died within the brief period already mentioned.

POST MORTEM.

The *post mortem* was very carefully performed, and revealed the following condition of the cerebro-spinal meninges.

The vessels covering the brain were generally and greatly congested, and at the base of the brain there was a very large effusion of fluid. The vessels of the membrane of the spinal cord were not so much congested as those of the brain, the congestion was in patches and the fluid effused was not so much in quantity as we expected to have found it.

The lungs were somewhat congested, but all the visceral organs appeared healthy except the liver, which was unusually dark in colour. The stomach and bowels were empty, and the right side of the heart was filled with very dark semi-fluid blood.

COMMENTS.

The resemblance in many, if not in most particulars, of this case to the one previously narrated is very remarkable, whilst the few differences in the symptoms and appearances—by these last I mean those presented both before and after death—are by no means irreconcilable with the disease being in both cases the same.

This youth had never had very good health, and like the young man G— G— had lived upon a diet of great plainness and doubtful wholesomeness.

I had nearly forgotten to remark that the swollen ankle referred to by Mr. Weston, and which he thought might have pus under the integuments, was not observable when I saw the case, and at the post mortem Mr. Weston cut down to and into the joint, but there was no appearance of pus or such a condition of the parts as would have been likely to lead to its formation; in fact, all was healthy.

When considering the above two cases as having occurred in this county sporadically, and connecting them as one must, I consider, inevitably do, as examples of the epidemic of so called spotted fever, which prevailed in the north of Germany last year, we must without doubt have our thoughts turned towards those epidemics of “Cerebro-Spinal Meningitis” which visited France and other coun-

tries many years back. The one which attracts the attention most, I think, would most probably be with others as with myself that described by Professor Forget in an account of an epidemic of Cerebro-Spinal Meningitis observed at the Medical Clinique of the University of Strasburg. It appeared in the *Gazette Médicale* April 9, 16, and 23, and May 7, and 14, 1842. Further notice of it was given in the *Gazette des Hopitaux*, June 2, 1842, but however strongly these descriptions may accord with those given by Dr. Mineir and recently by Stillè in regard to many of the symptoms, and most of the pathological conditions, yet there stands out the fact that Forget makes no reference whatever to the appearance of an eruption in any of his cases. The same observation holds good with regard to the description in the 10th volume of the *Memoires de l'Académie Royale de Médecine* for 1843, given by M. Rollet, of an epidemic of "Cerebro-Rachidian Meningitis and Encephalo Meningitis" which ravaged the garrisons of Versailles, Lyons, and Bayonne.

Considering that this authority describes the malady in the first instance as being "as rapid in its course as destructive in its results," the appended table of the result of his treatment seems almost too satisfactory, especially when M. Villar, in describing the same epidemic states that out of one hundred and fifty-four cases which he treated, sixty-six terminated fatally.

Are we to look upon these epidemics as being essentially of the same kind as those recently described by Gerhard and Lamb, and also by Wales:—the two former in the *American Journal of Medical Science*, July, 1863, and the latter in the same journal for January, 1864;—or are we to consider the spots (or eruption) so pathognomonic of the disease that their absence proclaims it as a different affection? To me it appears that the diseases are identical, and that the presence of the spots either before or after death—for they may appear at the one time or the other—is determined by some particular, but at present not well recognised condition of the patient.

I consider that the “spots” in my cases, and doubtlessly in those mentioned by the last named authors, were due to effusion of blood under the skin, for they were not elevated, and were in no way altered by pressure. Further, it is very well ascertained that there are maladies which receive their names from some well known characteristic symptoms; such symptoms, however, being in particular instances altogether absent. One need only refer to “Scarlet Fever without an eruption,” and to the different kinds of eruption seen in the three usual well known forms of the complaint, *Scarlatina Simplex*, *Anginosa*, and *Maligna*: but what influences prevail to produce the various types of the disorder we are, in our present state of knowledge, in no position to define. So with

regard to Cerebro-Spinal Meningitis, or so-called "Spotted Fever" of the epidemic kind. We are quite unable to say with any approach to certainty what occasions or prevents the appearance of the "spots:" certain peculiarities may be in some cases more pronounced according to sex, age, and idiosyncrasy: or the disease may be more or less governed by atmospherical conditions, or it even may be that the state of the organs of vegetative life at the time of the attack may exercise an influence in rendering milder or more severe certain symptoms of the disease.

All these then, or any of them, may operate in promoting or preventing the appearance of certain supposed essential features of a malady, but *how* they affect this is a problem yet to be solved.

We are not altogether without light on this subject. Presuming always that the disease now under consideration is a disease of the nervous centres, as its pathology and general symptoms would clearly indicate, nothing is more in character with sound observation than that in some cases of the malady there should be eruption on the skin.

The fact that lesions of the nervous system are followed, in certain instances, by cutaneous affections, as if of resultant character, was well and ably described by Mr. Paget, in the *Medical Times and Gazette* for March 26, 1864; and the same fact has also been observed and described by our American brethren in an admirable work pub-

lished in America later on in the same year, on "Injuries of the Nerves," by Dr. S. Weir Mitchell, Dr. G. E. Moorhouse, and Dr. William W. Keen, all of them Acting Assistant Surgeons in the United States Army, placed in charge of the wards for diseases of the nervous system—Turner's Lane Hospital, Philadelphia. The influence of nerve injuries on the cutaneous circulation, and on the production of cutaneous eruption, are described with a degree of precision rarely met with in science, and from a field of observation without a parallel.

These observers had, in fact, brought before them the worst cases of nerve injury resulting from the wounds of battle in the late strife, and of all the series of facts they have noted for us not one is more striking than the dependent relation of some cutaneous eruption on nervous lesion.

It does not follow, however, that every diseased condition, or injury of the nervous centres or tracts is productive of lesion of the skin. On the contrary, there are many severe nerve injuries which are *not* followed by such changes,—in short, the evidence points to the probable law that certain particular parts of the nervous system must be influenced in order that cutaneous indications should ensue.

If this evidence be reliable, we have no difficulty in assigning a reason why in one case of Cerebro-Spinal Meningitis there is eruption on the

skin and in another case no eruption. For as in no two cases are the lesions of the nervous system specifically the same in respect to their seat; as in one case the brain and in another case the spinal cord may be the part influenced, and as these parts in different cases are in their turn influenced differently, yielding different symptoms of a general kind, so must particular symptoms, such as eruption of the skin, vary according to the variation of lesion in the nervous tracts. For this same reason—viz.: difference as to seat of nervous injury, in some cases of the disorder there is no marked convulsion—no tetanus, but we could not thereupon define a case of Cerebro-Spinal Meningitis *without convulsion* as a case of a distinct or specific disease.

I need dwell no longer on this point. My object has been to show that the disorder is most correctly typified in its pathology, and that the symptoms, though in the main exact, cannot in every case be brought under due formula.

CAUSE OF THE DISEASE.

All the authors I have named as having written upon the subject of "Epidemic Cerebro-Spinal Meningitis," seem to be of one accord in connecting the proneness of individuals to attacks of this kind to exposure to bad weather or poverty of food, —in fact, to the causes which are universally

admitted as being likely to lower the tone of the system—the life-power of the individual—but no one that I am aware of has, as yet, ventured on any hypothesis as to its particular causation, except my friend Dr. Richardson, who has hinted at the possibility of its being the result of the ingestion of diseased grain—a sort of Ergotism.

In consequence of Dr. Richardson's idea with regard to the ingestion of unsound grain being *possibly* the cause of Epidemic Cerebro-Spinal Meningitis, I have been led to carry on a few experiments with animals fed upon damaged wheat, musty oats, ergot of rye and mouldy bread; the results I will briefly detail.

EXPERIMENTS.

I placed three rabbits in separate hutches—they were does, and each had had young ones some months previously to their coming into my possession.

Two of them were lively and well, but the third was dull—the coat was rough and the abdomen was larger than it ought to have been, and it had been purged. I shall, for convenience of reference, call this last animal number 1, and the others respectively 2 and 3.

I mixed, in a trough for each, the unsound grain and crumbled mouldy bread; I also placed in each hutch some good bread soaked in milk.

Numbers 2 and 3 at first seemed but little disposed to eat any of the food offered to them, but number 1 ate the unsound grain greedily from the first, scarcely touching the bread and milk. I observed, however, that it avoided the ergot and left, if not the whole, the greater part of it.

After a day or so numbers 2 and 3 commenced eating the bread and milk pretty freely, partaking of the unsound grain &c. only sparingly, and, like number 1, avoiding the ergot: things went on for two or three days much in the same way, but number 1, being much improved, ate ravenously of the milk and bread as well as of the grain, not even refusing the ergot. It was now not purged, but the abdomen continued to enlarge.

Numbers 2 and 3 were by this time eating of all the food provided for them tolerably well, but still not relishing the ergot. They began to look dull about the eyes, and the right eye of one, which I shall designate number 3, was inflamed and had a good deal of moisture about it. The coats of both looked staring, and they sat moping in the corners of their hutches, shewing but little disposition to move about—both of them were a good deal purged.

On the morning of the eighth day I found number 1 dead in the hutch; all four legs were stretched out to their fullest extent, and so straight that they looked like sticks, and felt as stiff as them too. The head was bent quite back upon the shoulders, the body being perfectly straight.

Upon opening the abdomen of this rabbit a considerable quantity of fluid escaped from the peritoneum; the lining membrane of the stomach and intestines was much inflamed and in the latter, *i.e.* intestines, there were some hard glandular looking bodies with small ulcerated spots upon them.

The stomach contained a good deal of undigested food, the intestines being nearly empty. The liver was enormously large and of such a dark colour that it might almost be called black. When incised it cut with remarkable firmness; the kidneys were of moderate size, but all their vessels were greatly distended with blood. The lungs were slightly, but only slightly, congested. The heart was healthy, but the cavities were quite empty.

I next laid open the head and found the membranes intensely congested; and this congested condition extended to the membranes of the spinal cord from one extremity of it to the other. There was a considerable amount of fluid effusion in the spinal canal and but little in the cranium.

The substance of the brain was soft; in fact, so soft that I had a difficulty in slicing it. The cord also was in a similar condition.

I remarked that the vessels of the conjunctiva as well as the vessels of the mucous membrane of the mouth and respiratory passages were unmistakably injected.

I examined this animal as soon after its death as possible, intending to keep numbers 2 and 3

some few days longer before killing them ; that is to say, if they did not die from disease as number 1 had died.

The other animals continued for the next four days in apparently much the same state as they were in when number 1 died, except that, if anything, they moved about even less than they then did, and when they moved at all it was done stiffly, and with a jerk something like a horse with string-halt, not with the natural elastic jump of a rabbit.

I now imagined that if their bad food was to give any token of its effects, time enough had elapsed to allow of such effects being seen on an examination, I therefore destroyed the animals by enclosing them in a box with chloroform at the bottom of it.

After death I first examined number 3, and as my attention had been attracted to the state of its right eye, I looked at this immediately, and found that the cornea was destroyed by ulceration and the contents of the anterior chamber had escaped—the limbs were not at all rigid.

I then opened the abdomen—the intestines were nearly empty ; the stomach contained a rather large amount of undigested food. The mucous membrane of the entire alimentary canal was very red and inflamed, but I saw none of the hard ulcerated glandular bodies that I met with in number 1.

The liver was natural in size but extremely dark in colour; it cut with tolerable, but not remarkable, firmness; the kidneys were moderate in size and without any appearance of congestion or inflammation about them; the lungs appeared healthy although somewhat congested; the heart was filled with semi-fluid blood of a dark colour.

The head was next opened and there was a strong contrast to the appearance seen in number 1; the membranes of the brain seemed in a perfectly healthy condition; there was no appearance of congestion, much less of inflammation and its results, and the brain substance was moderately firm. Upon examining the spinal cord and its membranes the vessels of the latter were found in an extreme state of congestion. The congestion was not general, the lower part being that in which the turgid condition of the vessels was most marked; there was no considerable amount of effusion.

I next examined number 2, and it will suffice to say that the abdominal and thoracic viscera were in the same condition as in number 3; but the membranes of the brain in this instance were decidedly more affected than those of the cord; there were well marked patches of congestion of larger and smaller size all over the cerebral membranes. The brain cut softer than in number 3, but nothing like so soft as in number 1. The vessels of the membranes of the spinal cord were pretty free from congestion, excepting at one spot about the termi-

nation of the cervical region; here there was as it were a streak of congestion about an inch in length: it seemed to commence and end abruptly.

These are the results of my first experiments, and they must be taken for what they are worth. That they prove to demonstration effects produced by diseased grain on the membranes of the brain and spinal cord, I have not the least intention of maintaining, but that they shadow forth some such a connection is, I think, a fair inference.

With regard to number 1 rabbit, it may very properly be remarked, that as it was ill at the time the grain was first given to it, there is a degree of uncertainty as to what occasioned the condition not only of the membranes of the brain and cord but of the softened state of these organs themselves. But with respect to numbers 2 and 3, there can be no doubt, I think, whatever; they were well, they became ill, and on inspection congestion of the cerebro-spinal meninges was plainly enough to be seen, although the parts affected were not precisely the same in the two animals.

TREATMENT OF CEREBRO-SPINAL MENINGITIS.

With regard to the treatment of this terribly fatal complaint, the various authors I have consulted agree principally in this, viz.:—that all rational modes of treatment are equally efficient, which means, I suppose, that none of them are of

any use. Everything at all likely to be of service seems to have been tried from the hot iron (actual cautery) of M. Rollet to extreme cold. Stimulants, sedatives, astringents, and cathartics, all appear to have met with the same success! and this state of things is likely to continue until such time as it may happen that by some method of experimental research, the true origin of the disease shall be disclosed.

HISTORY II.

A RARE CASE OF OVARIAN DISEASE.

Not many years ago Dr. Richard Bright, in an article on "Abdominal Tumours," described cystic dropsy of the ovary as a "malignant disease," and this he did, without doubt, not from any reference to its pathological characters, but from its acknowledged incurability and fatality.

It may be gathered from the tables of Dr. Robert Lee and Mr. Safford Lee, that the mortality of the disease is at the rate of about eighty or ninety per cent. in the course of two, or at the most three, years, in all those cases that are in a "progressive" condition; whilst in the very few that are in a "stationary or chronic state," the chance of a longer duration of life is somewhat more favourable; but with these there must always necessarily be the risk of their becoming in an active condition at almost any moment.

In this very fatal affection there sometimes have happened instances of cure by spontaneous, or accidental, rupture of the cyst or cysts. This is a fact that is well known, although generally such an

occurrence has proved the death of the patient. But, *as far as I am aware*, there is no recorded instance in which the contents have been discharged through the stomach and the patient has recovered. Moreover, I can find no reference in any of the authorities I have consulted, nor can I learn from any of my professional friends that they ever knew or read of any such case having happened. I am disposed, therefore, to regard the following instance as being perfectly "unique."

CASE.

The case occurred in my practice at the Stafford General Infirmary.

C—C—, a married woman, having borne several children—aged 43—was admitted on February 3rd, 1862, suffering from ovarian dropsy. The disease, she said, commenced about three or four years previously, appearing at first as a tumour on the right side near the groin, and in the region of the ovary. The tumour gradually got larger, and then, for a time, appeared at a stand-still; but for the six or eight months prior to her admission she rapidly increased in size, and on coming to the Institution she presented a very jaded and cachectic appearance; the lower limbs were in a state of anasarca, whilst the abdominal walls were distended to an immense extent. Upon being manipulated, the parieties seemed to have little or no play over the tumour, which oc-

cupied pretty equally the whole of the abdomen, and evidently contained fluid. When sitting, the right side seemed larger than the left, but not very much so; the surface of the tumour was uneven, and there were several "sulci," or grooves, plainly to be felt; her bowels were spoken of as being obstinately constipated. She was, at times, in great pain, and had no appetite; her sleep was very uncertain and disturbed, and her breathing so difficult that she was obliged to use the bed-rest. The pulse was thin, weak, and rapid—about ninety-eight—and her menstruation was described as being irregular, painful, and the menstrual fluid somewhat offensive in odour.

The diagnosis arrived at was that of "*multilocular tumour*" with considerable adhesions. She expressed a strong repugnance to any operative interference, and it was decided, after taking all the circumstances of the case into consideration, not to press the matter.

She had prescribed for her chlorodyne at night, which had the effect of soothing and relieving her sufferings in a very satisfactory manner: the constipation was obviated sometimes by the use of Podophyllin, sometimes by castor oil, and sometimes by a simple enema. Her diet was such as her stomach—which was irritable—could bear and make use of.

Her urine was examined, and gave no indications of the presence of albumen; it was high

coloured; the Sp: gravity was 1.025, the reaction was acid; the urine usually deposited large quantities of lithates.

She continued in much the same state as when admitted, but weaker if anything, until March the 8th. During this day she complained of great pain in both the abdominal and epigastric regions, and she said that she had suffered in a similar way before. On the night of the 8th, or rather early on the morning of the 9th, she became violently sick, the previously mentioned pains being greatly aggravated.

Soon after feeling sick she began throwing up large quantities of a glutinous thick fluid, somewhat dark green in colour, exactly resembling that which I have seen follow the tapping of an ovarian cyst. The sickness continued for twelve days, during which period we computed that she could not have parted with less than two or three gallons of fluid: this we felt certain was quite within the mark, but, as may be imagined, it was not possible to fix the quantity with anything like certainty; it very probably was much more, but certainly was not less.

In the fluid vomited there was occasionally to be seen a flaky kind of material, not unlike coagulated lymph, and sometimes small masses of more solid matter. As during the whole of this time every particle of food, of whatever kind, was rejected almost as soon as swallowed, she was nou-

rished by means of beef tea, injections, &c. Dilute hydrocyanic acid with saline effervescent, &c., were prescribed, in the first instance, with the view of allaying the sickness, but they were of no use, and indeed seemed rather to increase than diminish the nausea, and she continued to throw up everything for the time already named.

Her circumference became amazingly diminished and on the 21st her vomiting ceased, and her stomach retained a little food. From this date till the 27th she gradually and steadily improved, taking light and easily digested food with evident relish; her sleep became tolerably good, and the expression of her countenance was remarkably altered for the better; the pulse, which had been at times almost imperceptible, got gradually stronger and firmer, and her bowels acted without the assistance of an aperient, so that I began to be hopeful that she would have no further immediate danger to encounter. But on the 28th she commenced with a sudden and acute attack of double pneumonia, confined however very much to the lower lobes of the lungs. Her cough was incessant, with a copious expectoration of rusty-coloured sputa. Her pulse again became weak and rapid, and altogether her state was as unfavourable as it very well could be. She had given to her for many hours large doses of acetate of ammonia, plenty of beef tea and wine, and at length, as her symptoms seemed in no way altering for the better, I had her chest, front,

back, and sides wrapped up in a warm linseed meal poultice. The relief afforded by this application was most marked, and almost instantaneous. She went to sleep soon afterwards, and slept for several hours, and on waking was evidently better. I directed the poultice to be continued for some days.

Subsequently she had no unfavourable symptom, and daily improved up to the time she left the Infirmary, which she did on the 1st of May, in a state that might fairly be considered one of convalescence. There was still a tumour on the right side, midway between the umbilicus and the groin. It was about the size of a large orange, of an irregular surface, and painful when pressed upon.

The areolar tissue of her legs contained fluid, but nothing at all to be compared with what it did when I first saw her. She attended as an out-patient for about two months, her anasarca diminishing, and the tumour also getting less.

When she ceased attending the Infirmary I lost sight of her for some time, and in 1864 one of the nurses informed me that she had recently died, *not* however from *ocarian disease*, but from fever. No *post mortem* I understood was made, and this is, of course, to be regretted.

At the time she was throwing up the contents of the cyst, I examined, microscopically, the lymph-like flakes that I spoke of. They were structureless in appearance, and I could not find anything at all

resembling an acephalocyst. The fluid was loaded with albumen and had a sp: gravity of 1.022, and sometimes much higher; at the onset it was too thick to allow of the ready use of the hydrometer.

COMMENTS.


Beyond the manner in which this case terminated there is, perhaps, not much to distinguish it from the many instances which might be quoted of nature effecting her own cure, under circumstances the most difficult and unpromising: but the recent struggle which has been going on amongst obstetric authorities as to the advisability or not of removing ovarian tumours by an operation which one of our great surgical authorities not very classically designated "*belly ripping*," renders some few comments almost unavoidable.

What the result of an operation on the patient whose history I have been giving would have been one cannot very well venture to say, but of this we may be certain, that but for the spontaneous rupture of the cyst she would ere long have been, as hundreds have been before under similar circumstances, worn out by suffering, dying a lingering, painful, and miserable death: nothing short of an operation could have saved her, and that probably, or even possibly, might not have done so. If it "probably" *would* have done so, why, of course, there could be no doubt as to its advisability; even

if it "*possibly*" would have done so, I cannot see good grounds for coming to any other conclusion as to the "*advisability*." But in *such* a case it is, as well, and perhaps better, to leave the patient to make her own choice.

There must be cases in which those skilful in arriving at a correct diagnosis, and equally skilful in the performance of ovariectomy, should alone decide whether or not to *recommend* the performance of it, and no sensible woman, having confidence in the source from which such recommendation comes, would have any hesitation, I should think, in being guided by it, especially, too—the operation being thought desirable—when the successfulness of it can be so amply proved by a record of cases such as, fortunately, it is in the power of Mr. Spencer Wells, Mr. Clay, Mr. Brown, and others to furnish.

That any one in the profession should now endeavour to maintain an argument adverse to the operation, in certain carefully selected cases, is only an instance of that wrong-headedness sometimes seen in minds which, in every other respect, are free from prejudice. But proven and ascertained facts must, *even with these*, eventually prevail; and the argument used by Mr. Erichsen at a meeting of the Royal Medico-Chirurgical Society, during the discussion of a very admirable paper of Mr. Spencer Wells on ovariectomy, may fairly be taken as conclusive on the subject.



He said, "it is old and trodden ground to compare ovariectomy with the result of the operations for hernia, ligature of arteries, &c., and in these cases also the comparison is scarcely fair, as these are operations of necessity, whilst ovariectomy is an operation of expediency, and not of immediate and imperative necessity; but, compare it with 'amputations of expediency,' he would take for this purpose the statistics of a most able paper published two years ago in the 'Transactions' of this Society, giving the results of amputations performed in one of the largest hospitals in London—Guy's.

"In that paper Mr. Bryant stated that the mortality after amputation of the lower extremity for tumours was thirty-six per cent., and 'amputations of expediency' of the leg was sixty-eight per cent. Compare this result of amputation performed under the most favourable circumstances by men of the greatest skill and judgment, with those of ovariectomy, and the advantageous position of the latter operation is at once seen."*

The treatment by tapping and injections, although occasionally attended with a fortunate result, has not been such as to produce any favourable impression with medical men generally, and indeed until the statistics afforded by the gentlemen before referred to had placed the radical cure of this disease in a favourable light, any one having ovarian

* The mortality of ovariectomy is estimated by Dr. Graily Hewitt at sixty-five per cent. See his work on Diseases of Women, page 590.

dropsy was looked upon as being in a hopeless condition, and the means used were confined to those of mere palliation. In fact, in this as in other cases, where alteration of ultimate organic structure was confirmed, the attempt to "*cure*" must have only been looked upon as the effect of either ignorance or want of judgment.

But now it is satisfactorily demonstrated that a positive and absolute cure may be accomplished by the entire removal of the diseased organ, although such removal may, nay, must, be attended by special dangers; so that, whilst the disease must even yet be considered one of the most fatal that a woman can be afflicted with, still it is not now what it once was—"a case without hope."

It is, I conceive, as much the duty of the physician to prevent as to cure the diseases of our species, and happily the instances in which he can fulfil the former are by no means scarce. But as regards "ovarian dropsy," I do not know that any effort has ever been made in this direction; so that to the enquiry, Can nothing be done to prevent it? the only answer deigned in reply is an echo—Can nothing be done to prevent it?

HISTORY III.

ON SECONDARY CANCER IN THE LUNG.

Any more fearful or painful malady than that of "cancer" does not exist in the whole range of diseases with which the profession has to deal, and its history, forms, causes, course, and terminations must ever be with physicians and surgeons alike subject matter of the keenest interest. As there is, at present, a growing disposition with some to regard the disease in the light of (at the commencement) a purely local affection rather than as resulting from that state of constitution usually expressed as a "cancerous diathesis," there is thus lent an increased motive for accumulating any evidence that may be at all likely to bear, either directly or indirectly, upon the elucidation of a subject beset, as this undoubtedly is, with difficulties both numerous and great.

The works of Walshe, of Stokes, of Graves, of Lebert, of Lisfranc, and others, excellent and exhaustive as they may appear to be on this particular point, only iterate and reiterate the unproven idea, that a peculiar constitutional condi-

tion favours the growth of an adventitious product through the medium of a favourable "blastema:" and, if they do not altogether ignore, they but slightly hint at the probability of a dissemination of the disease (*ab initio*) by means of some one solitary local and particular "nidus." In fact, the dictum of Walshe, that "a cancerous tumour, under all circumstances, even should it remain single and stationary for years, is but the local evidence of a general vitiation of the system," seems to have been the "creed" to which most writers upon the subject have pinned their faith. From the plausibility of the notion, and the easiness of its application, one can scarcely wonder that it should have so many advocates; but that there is another side to the question, and that this other side possesses numerous points, admitting not only of argument but of proof in its favour, was made plainly evident in the paper read before the British Medical Association at the last meeting, held at Leamington, wherein the writer—Mr. Moore—clearly made out his views by the most fair, and, I think, unanswerable arguments as to the *origin* of the disease being purely local in its nature. Nor does Mr. Moore stand alone in this opinion: it did not originate with him, although I consider great credit is due to him for the able and clear way in which he placed the "pros" and "cons" before the profession.

At or about the time that Walshe penned the

lines I have recently quoted, there appeared a work (published at Copenhagen) by Dr. Hannover, entitled “Den Pathologiske Anatomie’s Svar paa Spørgsmaalet—Hvad er Cancer?” *i.e.*, the Response of Pathological Anatomy to the Question—What is Cancer? in which the whole of this subject is most ably handled, and the adherence of the learned writer given to the local origin of the disease in words to this effect: “when cancer remains long stationary in only one part of the body, it has no positive title to be considered a constitutional disease.” The local origin of cancer was also firmly held by the late Dr. Snow.

Until about the year 1840 the frequency of cancer of the lung was but little recognised, and although its occurrence, either as a primary or secondary affection, is now admitted to be a matter of no very great rarity, yet instances of it, even in the secondary form (the more common of the two), are by no means so frequent as to remove it altogether, I think, from the category of rare diseases. The two following cases of it, which occurred in the Stafford General Infirmary, have about them some features which induce me to give their history:—

CASE THE FIRST.

C— M—, aged 14, was admitted under Dr. Masfen, one of the surgeons to the Infirmary,

on August 24th, 1864. His general health had been for the most part good, although his habits had been rather wild. He was of a fair complexion, with light blue eyes and light brown hair, a long and somewhat full and prominent upper lip; altogether he had the look which is usually called "strumous."

About two years before being admitted into the Infirmary, his left leg became, without any known reason for so doing, hot, swollen, and painful. After a short time, and in the absence of any treatment, the swelling subsided, leaving a hard lump of a red colour near the centre of the shaft of the tibia. It was about the size of a walnut. This gradually got larger, and, when admitted, the leg measured—over the tumour—twenty inches in circumference.

The diagnosis formed by the surgeons was "medullary cancer," and the limb was amputated at the lower third of the femur on September the 3rd. From this time to the 30th of the month the healing of the stump appeared to progress favourably, but on the 2nd of October there was an unfavourable change. He did not seem so well as he had done since the performance of the operation, and the discharge from the stump was not healthy. Besides this, a dark claret-coloured tumour made its appearance at the lower and fleshy edge of the flap. From this date to the 13th of the month he got decidedly weaker, and on this day he had a kind of convulsive fit, accompanied by great

shortness of breath. Upon recovering a little, he broke out into a most profuse perspiration; his pulse, which before this attack had been quick and irritable, became now more so, 130 beats in the minute, and his tongue was dry and red. On the 14th he had another fit, resembling in every way the one he had the day previously. He perspired very much during each night, but on the 16th, the night sweating abated, his pulse became quieter, and his tongue moist and clear. From this date he began to improve slowly, and the stump healed, with the exception of the spot occupied by the dark claret-coloured tumour before mentioned. He was now discharged, but did not actually leave the Infirmary.

I am indebted for the above particulars to Mr. Greaves, the house surgeon.

On the 24th of November, 1864, the boy was placed under my care. He was complaining of intense pain in the chest, darting, as he described it, from the front and sides of his ribs towards the spine. He was deadly pale, and suffered from great dyspnoea. The conjunctiva and mucous membrane of the lips were perfectly blanched, and his whole appearance was terribly anæmic; his tongue was moist, but with only the faintest imaginable shade of redness; the pulse was thin, indeed thread-like, and rapid—120.

I was not, on first seeing him, made acquainted with his previous illness and the operation he had undergone. I examined his chest, which appeared

well formed, but covered with very little flesh: the skin was of marble-like whiteness—that is to say, the integument had this appearance; the intercostal spaces seemed a good deal depressed; the two sides were alike, and expanded but little, although equally. The respiration was 29 in the minute, the expiration being both too prolonged and too pronounced; there was some little dulness under the clavicle on the right side, but none on the left; posteriorly there was great dulness from the summit to the base of both lungs; vesicular respiration was heard but very indistinctly at any point of the chest anteriorly, and could not be heard at all posteriorly, whilst tubular breathing was heard much beyond its natural limits, and upon applying the hand to the chest, the vibration occasioned by speaking and coughing appeared exaggerated on both sides alike. A slight blowing murmur accompanied the first sound of the heart, and under each clavicle there was a very decided subclavian murmur. This, I have before noticed, is no uncommon sound in cases of anæmia. He had a cough, but by no means a severe one, and expectorated but very little, and what he did spit up was a glairy kind of fluid untinged by blood.

Not having been made acquainted with the boy's previous history, I at first began to think he might be suffering from leucocythemia with pulmonary congestion, but, upon raising him for the purpose of my examination, I was warned by the

nurse "not to hurt the stump of his leg," and then, and not till then, I became acquainted with his antecedents. Of course, the nature of his malady was now no mere matter of conjecture, and as I do not know of any author who has yet been sufficiently indiscreet to lay down a precise method of treatment for this disease, I do not feel ashamed to own that mine consisted simply of attempts at palliation, and promoting what Dr. Watson, in some observations of his on the treatment of malignant stricture of the œsophagus, classically terms, and what Arbuthnott prayed for, the "euthanasia."

The poor fellow got rapidly worse, sometimes appearing a little easier, but always being in suffering; his difficulty of breathing became greater and greater, until he was released by death on the morning of the 8th of December, 1864.

POST MORTEM.

The head being opened the brain and meninges were found to be healthy; the vessels, however, contained but little blood, and the lateral ventricles had more fluid in them than is usual in this position.

The thorax was next examined, and some few lax pleuritic adhesions brought into view. The cervical glands appeared in no way affected, neither was any cancerous material to be perceived in the anterior mediastinum, but the entire substance of

both lungs was pretty equally filled, or infiltrated, by masses of cancerous matter, encephaloid in character, and so charged with melanotic substance that it gave the organs a dark red look, in fact, so dark was the red that it might almost have been called black.

The lining membrane of the bronchial tubes was but slightly moist, and the colour was nearly that of mahogany.

The posterior portion of both lungs was so completely made up of these melanotic masses, that it might properly be said to be one mass of them.

The juice, or fluid, expressed from these cancerous bodies afforded, when examined microscopically, an immense number of irregularly shaped cancer cells with double nuclei, and there were also numerous discoid granular looking bodies, which I felt disposed to regard as being white blood corpuscles, or cells; and the same kind of cells were observed in the fluid obtained from other organs and parts.

The stomach was remarkably pale in colour, but without any evidence of disease: it contained a small quantity of food. The liver, kidneys, spleen, pancreas, and the whole intestinal canal were quite healthy, but much paler in colour than they are usually seen to be.

The amputated stump was imperfectly healed, and the dark claret-coloured tumour before spoken of, yielded, upon pressure, the same characteristic

fluid that I obtained from the "masses" in the lungs.

COMMENTS.

It is difficult to decide whether this cancerous condition of the pulmonary organs was at all in existence at the time of the operation, but I was informed by Dr. Masfen that a very careful examination of the lungs had been made prior to the operation, and no detectable symptom of lung mischief was noticed. Nevertheless it might even then have commenced, although not making itself evident by any of those symptoms usually met with in more advanced cases; in fact, one would think it most probable that it must have made at that time some progress, for it seems but little likely that these organs could have become such a huge depository of cancerous disease within the space of something under three months: on the other hand, it must not be forgotten, that all the soft kinds of cancer—and this was indubitably encephaloid—are prone to rapid development; that every proper care had been taken to endeavour to detect pulmonary disease if present, and that no suspicion existed at the time of the examination, or even subsequently, until within a few days of his being placed under my care, that these important organs were suffering from any mischief whatever.

The next case, which commenced like the one just narrated, as one of Surgical Cancer, also occurred in the Stafford General Infirmary, and was under the care of the same gentleman, Dr. Masfen.

CASE THE SECOND.

W— B—, a farm labourer, aged 44, was first admitted at the latter end of the year 1863. His general health had been, previous to his present illness, good: he was of a fair complexion with light blue eyes, his habits had been steady, and he had a good and comfortable home. In the month of June in this year he had occasion to work for three or four days in water, and immediately afterwards began to experience a sensation of pain in the left knee; this continued for some three months, sometimes being more and sometimes less painful: it now, however, in addition to being painful began to swell slowly, and this swelling gradually increased up to the time of his admission.

He remained in the Infirmary about two months and then returned home, his knee being larger than when admitted but scarcely so painful.

On the 8th of April, 1864, he was re-admitted, the knee having become much larger and intensely painful; it now measured twenty-two inches in

circumference, and it was decided to amputate, the operation being performed at the middle third of the femur, on May 17, 1864.

The stump was said to have gone on well from the first, and as it was nearly healed on July the 22nd, he was then discharged.

There was some little uncertainty in the minds of the Surgical Staff as to the character of the disease found in the bones of the joint, and it was, in consequence of this, sent to Dr. Wilks, of Guy's Hospital, for his examination and opinion. The latter I here give verbatim.

Dr. Wilks said, "I have generally observed that such diseases have occurred in young people, and I am under the impression mostly in girls.

"It affords a good example of a fact which I have long been preaching, that you cannot divide all new growths into cancer on the one hand, and simply innocent on the other, there are grades of malignancy between them.

"If a firm fibrous tumour should generally imply innocency, and a soft brain-like structure malignancy, you have in your case something intermediate; again, if a tumour being circumscribed suggests an innocent growth, and its invasion of other tissues a malignant one, you have both characters here.

"Its external part is circumscribed, and apparently is confined to the neighbourhood of the bone, yet the section shows that the shaft of the

femur has been destroyed. The microscope, too, shows plainly plenty of nucleated cells and large nuclei, indicating its proneness to rapid growth, but at the same time some of the nuclei belong to fibres which are found in more innocent tumours.

"I should call the growth which has involved the bone a malignant one, but not possessing the highest character of malignancy.

"I think the growth, if it had been isolated on the surface of the body and removed by the Surgeon, would probably have been called 'recurrent fibroid'—prognosis bad."

From this carefully expressed statement we can feel but little, I had better say no hesitation in putting down the disease as belonging to the class usually called cancerous, or malignant, whilst the subsequent history of the patient only tends to confirm the accuracy of the opinion.

From the date of his discharge he continued to attend for some months at the Infirmary as an "out-patient," the stump in the meantime becoming perfectly healed.

Soon after he left the Infirmary—that is, on July the 22nd, 1864—he complained occasionally of pains in his chest, and he had a slight but not troublesome cough. He resided in the neighbourhood of Uttoxeter, some fifteen miles from Stafford, and as towards the end of the year he became more of an invalid, he felt unequal to take such long journeys, and placed himself under the care of a

medical gentleman in his own locality, Mr. Hawthorne. It is partly from Mr. Hawthorne and partly from the man's own relatives, that the remainder of the account of his last illness and death is derived.

When the winter came on his cough increased very much, and the pains in his chest assumed a much more severe character; he described them as being of a darting kind, and that they seemed to commence at the sternum and to pass from thence towards the spine. He now got very short of breath and, using the language of his friends, was as "white as wax," gradually getting worse. He commenced after a time to have some scanty expectoration; this was at first thick and glairy, a little further on the expectoration altered and was like currant jelly, its odour not being very agreeable, subsequently it had a greenish-brown tinge, and was terribly offensive.

Almost daily his symptoms became aggravated and his sufferings more intense. He died on the 23rd of July, 1865, the cause of his death being, Mr. Hawthorne said, "ulcerative cancer of the lungs."

I feel considerable regret at not being able to give as I wished to have done, and expected to be able to do, a more minute and detailed account of the latter part of the history of this case. Sufficient, however, has been offered to render indubitable the nature of his disease and the cause of his death.

COMMENTS.

In the admirable work of Mr. Travers "On Constitutional Irritation," he terms "medullary cancer" the cancer of scrofula, and certainly the family history of the boy C. M. and his own personal "physique" would make it appear that, at least, in his case the connection had been made good. But the "antecedents of cancer" are as obscure in this our day as when the subject was treated of years past by Cruveilhier, by Andral, by Carswell, by Abernethy, by Broussais, and the host of other equally talented writers, whose works have proved their interest and their industry in the investigation of this disease and its surroundings.

What is it that regulates the production of what Bennett would designate molecular cancerous material: what Walshe and Müller called the cancerous "Cytoblast:" and what Beale would consider as the "germinal matter" and "formed material" of cancerous elements?

Can it be the result of an error of selection? every organ—every tissue—every part of every organ and of every tissue should both select and appropriate the proper quality and quantity of material for its support and restoration, an error of *appropriation* in the shape of superfluity in quantity, however correct in quality, might lead simply to excessive *analogous* formation, whilst an error in

selection would conduce to the growth of a *heterologous* product. It is but an idea. Is it worth a further thought?

“Cancer,” said Mr. Moore, in the paper I have already referred to, “is eminently a disease of persons whose previous life has been healthy, and whose nutritive vigour gives them otherwise a prospect of long life;” and this, as a rule, is without doubt a well supported fact, and so the question yet awaits its answer. And that question it must be remembered still is, not what *is* Cancer, but what *occasions* it?

There is no proof whatever that the disease has ever been produced by contagion or infection. The experiment has been tried to communicate it by inoculation, and cancerous matter has been injected into the veins without accomplishing the expected effect. In this respect cancer is unlike tubercle, which has been proved to be inoculable by M. Villemin, whose experiments upon the subject were communicated to the Academy of Sciences, and an account of them given in the *Gazette Medicale de Paris*, December 16th, 1865.

In the Session 1859-1860, Dr. Richardson placed before the Medical Society of London a series of experiments, in which attempts were made to induce cancer by injecting cancer juice and dissolved cancer matter, not into the blood but into the tissues of animals.

These experiments have not before been pub-

lished, and as their author has placed the original reports of them in my hands "to do with them whatever I like," I introduce them here as specially bearing on the point under consideration.

REPORT OF EXPERIMENTS ON THE SYNTHESIS OF CANCER.

Dr. Richardson says, "The object I had in view in the performance of the experiments about to be related, was to determine whether the fluid that can be expressed from cancer growths is capable of producing cancer locally, in the healthy tissues of animals.

"Through the kindness of Dr. William Willis, House Physician at the Middlesex Hospital, I was supplied immediately after its removal with a human spleen, one half of which was affected with medullary cancer.

"EXPERIMENT NO. 1.

"From the cancer mass, while it was in the fresh state, about three drachms of a whitish milky fluid was obtained by expression. The fluid was now injected with a syringe through a fine hollow needle into the loose cellular tissue in the flank of a large and healthy rabbit. The operation appeared to give no pain, and the whole of the solution was retained. At first there was a soft swelling of the size of a large hazel nut, but in a few hours this passed away."

“ Twenty-four hours after the operation, on feeling the part, there was obviously a circular ring of hard tissue of the size of a half-crown piece: the ring had the thickness of an eighth of an inch, and inside it the structure was soft and boggy. Gradually the whole mass became hard, and eight days after it showed a tendency to slough in the centre.

“ On the twelfth day there was open ulceration from the centre with large granulations, and the ulceration continued for seven weeks from the period of operation. By this time the whole of the hard structure was removed by the ulceration, and now there was left a deep opening, elliptical in shape and a full inch in diameter.

“ Healing commenced at the narrowest part of the ellipse and progressed favourably but slowly.

“ Three months after the injection the sore was quite healed, but the cicatrix was not firm. The line of the cicatrix was puckered.

“EXPERIMENT NO. 2.

“ The cancer mass, from which no more liquid could be expressed, was removed from the healthy part of the spleen and was cut up into very small parts (minced).

“ The mass was now placed in a large glass globe and was treated with 1,000 grains of distilled water, containing twenty grains of pure ammonia. The globe was then placed in a temperature of 98° Fahrenheit, and kept at that temperature for three

days, its contents being frequently agitated. It was thus brought into a state of almost perfect solution and was filtered through linen, and only a few fibres were left on the linen as residue. The fluid was dirty white in colour, and was perfectly free from putrefaction, but it had an odour of the ammonia.

“ Three fluid drachms of the fluid thus obtained were placed in a flask and exposed to a temperature of 98° Fahrenheit, until the smell of ammonia was no longer perceptible. The loss of water was made up, by the careful addition of distilled water, to three fluid drachms. The fluid thus obtained was dirty white in colour, and of feeble alkaline reaction free of all smell.

“ The fluid was injected into the cellular tissue of another rabbit in the same part of the body. The same instrument was used for the injection. The disease produced followed, in symptoms, so closely that observed in the former experiment that no new description is required.

“EXPERIMENT NO. 3.

“ Three drachms more of the fluid derived, as in the last experiment, and prepared in the same manner, were injected into the cellular tissue of a young rabbit in the same position.

“ The symptoms of hardness did not in this case appear, but the part sloughed for the space of half an inch and then healed rapidly. The wound

was quite well in a period of four weeks from the operation.

“EXPERIMENT NO. 4.

“The secretions from the open sores of rabbits 2 and 3 were collected until they amounted to three fluid drachms.

“The fluids were caught in a test tube which was charged with a grain of ammonia, and the quantity was yielded by collecting it night and morning for four days. The fluid was warmed as before, to 98°, to get rid of the ammonia, and water was added to make up loss. It was then injected into the cellular tissue of the flank of another large rabbit.

“In this case the immediate swelling did not subside, but there was much heat and rapid ulceration. The ulcer healed in three weeks.

“All these rabbits ultimately recovered perfectly. With regard to the first two rabbits the greatest difference of opinion prevailed amongst those who saw them, as to the nature of the disease produced. At one stage, when the edges of the ulcers were very hard, it seemed as though scirrhous ulceration had actually been set up, but the ultimate perfect healing removed this suspicion, and led me to the conclusion that nothing more than a prolonged and low form of ulceration had been induced. I have always considered these experiments as negative, and yet they are some-

thing more. They shew that when cancer matter is injected into the living tissue it leads to a process of ulceration, which is characterised by no acuteness of inflammatory action, but by a very slow recovery.

“Those who believe in the constitutional, or systemic, origin of cancer will, of course, plead that the local manifestation of the disorder was in these instances produced, but, that the local mischief was recovered from because there was no constitutional taint behind to back it up. Those who believe in the purely local origin of the malady, will urge that the proper local propagating plasma was not used, or, that the plasma was not thrown into the tissue at a point where it could excite specific morbid action.

“For my part I am not sufficiently advanced to pronounce a judgment on these debateable points, and I am therefore content to give simply the results which nature has placed before me.”

These experiments, so ably devised and so judiciously carried out, must be looked upon, I conceive, by all unprejudiced persons as being negative in their character, just, indeed, as Dr. Richardson has himself considered them to be; but whilst it must be admitted that there is the possibility of some of the advocates of the constitutional theory of the origin of cancer endeavouring to hang what they consider to be a proof in their favour, on the circumstance of the ulceration and its

accompanying hardened edges, it will be well for them to bear in mind the means by which nature repairs such injuries as those inflicted on these rabbits, and in gramnivorous animals generally. It is, I believe, a fact, that with creatures of this class, wounds, sub-tegumentary in character, *never* heal by what we understand as the "*first intention*," but, on the contrary, healing is accomplished by the process which veterinarians of the "old school" used to call "*healing from the bottom*," that is, by suppuration and granulation.

Such being the case, the method of reparation carried on in rabbits 1 and 2 presents no peculiarity whatever beyond the slowness of the process.

Of course, it might be said that the result would have been different had the experiments been performed on human subjects instead of animals: but such an argument (which is only another way of saying what Dr. Richardson suggests) must stand for nothing as far as these experiments are concerned. They were planned and carried out to see if the disease could be induced in animals by the introduction of cancer material from human beings. Cancer was *not* produced. The same kind of experiment performed on similar animals by M. Villemin with tubercular matter originated in each instance the identical disease. There can be but one conclusion—"tubercle" is inoculable, "cancer" is *not*.

The opinion of Walshe and others that several

organs or parts, may be, in the first instance, simultaneously attacked with cancerous disease, or, in other words, that primary multiple cancerous tumours occur at one and the same time, is neither more nor less than an unsupported assertion, and the most close and scrutinizing observations made in the present day have failed to afford any instance of such an occurrence: whilst any cases that have been quoted from times past appear to rest for proof on what Mr. Moore has correctly phrased "traditional authority" only, and thus the hitherto attractive and easily assumed constitutional theory must be content to have its opponents who cannot be satisfied to adopt it, because they believe it to be inconsistent with present established facts.

Van Kleffens remarks that cancer of the lungs is a disease that has been observed at all periods of life from *twenty* to *seventy*, and that it is most frequent between twenty and thirty, the encephaloid form of the disease being the most common. It will be remembered that the boy C— M— was only fourteen, and in this respect the case seems singular, for I cannot find an authority that distinctly mentions an instance of *secondary* cancer of the lung in any one so young as he was.

Injuries have been frequently considered, and not unfrequently admitted, as the *exciting* causes of an attack of cancer.

W— B— afforded no evidence whatever in himself, neither was there any in his family, of a

predisposition (if there be such a thing) to cancer, and certainly the occurrence of the pain in his knee immediately after, if, indeed, it did not actually take place whilst working in water, creates a suspicion that this circumstance might have provoked the commencement of his disease.

As to the easiness or difficulty of diagnosing cancer of the lung, the various authors who have written upon the subject seem to be anything but of one accord, which is the more surprising, as they are nearly all equally able men, and have certainly had ample opportunities of forming their opinion.

Walshe and some others proclaim the certainty and facility of the proceeding; Stokes, Graves, and Hughes, admit that there is at times a difficulty, whilst Bennett, in his "Practice of Medicine," says that where there are "disseminated nodules" there are neither physical signs nor functional symptoms to indicate the presence of cancer:—and further on he remarks, that "It must be confessed, however, that notwithstanding the valuable labours of Stokes, Hughes, M'Donnell, Walshe, and Kilgour, the means of diagnosing this lesion are very uncertain." He then gives a series of cases, and when descanting thereon adds—"But in none did any combination or succession of signs exist which could induce anyone to pronounce that pulmonary cancer was present."

In a small but admirably written work on Intra-thoracic Cancer by Dr. Cockle, Physician to

the Royal Free Hospital, and published last year by Churchill, there are some exceedingly interesting observations on Intra-thoracic Cancer generally, and Pulmonary Cancer in particular.

Dr. Cockle, whilst evidently thoroughly acquainted with the opinions of the very numerous and distinguished writers, of all countries, who have contributed to the literature of this subject, does not hesitate to express, in a very candid manner, *his own opinion*, and most certainly the attention he has given to the subject, fully entitles him to do so authoritatively. When, alluding to the diagnosis of this disease he quotes from Baglivi "O! quantum difficile dignoscere morbos pulmonum; quantum difficilior eos sanare," and then continues "His words have echoed on, even to ourselves, despite the aids furnished in the department of physical diagnosis, and we never, probably, feel more sensible of their truth than when dealing, practically, with the subject of Cancer of the Lungs."

Speaking from my own experience, I should be disposed to adopt the views of Doctors Bennett and Cockle. Of course, if with certain pulmonary signs and symptoms there be indications of cancerous formations in other parts, a *crutch* is given to the diagnosis which makes it easy: but in the absence of such additional evidence, I have met with not a few instances in which the diagnosis might very properly be called difficult and uncertain.

HISTORY IV.

ON THE TREATMENT OF ACUTE RHEUMATISM BY BLISTERS.

There are but few ailments which have yielded more unwilling, uncertain, or unsatisfactory answers to our pathological enquiries than Acute Rheumatism. This is best exemplified by a reference to any work written in modern times upon the subject. It will then be found that the pathology is summed up, if not in these words, in words to this effect:—"Rheumatism is essentially a blood disease, and the blood poison is suspected to be lactic acid. The blood has also been said to contain a considerable excess of fibrine; the urine is intensely acid and very high-coloured, and also has, as one of its contents, much uric acid. There is likewise an excess of urea." This is about the sum total of the whole that is known, or rather believed to be known, of the pathology of acute rheumatism. Can anything be more vague or more unsatisfactory? No wonder at the growing impatience displayed for more accurate knowledge as to the essential cause of the disorder. It is pro-

bable that the discovery (if such it may be called) of the American writers whose works on injuries of the nerves I have already had occasion to refer to, of the connection between spinal nerve lesion and an arthritic rheumatic condition may point the way to a definite and perfect unfolding of that which is now wrapped in doubt and mystery.

It would be exceedingly difficult to find any greater or stronger proof of the uncertainty that prevails in the minds of medical men, as to what is the positive nature of the blood poison which exists in rheumatism, than the varieties of treatment which have been and still are proposed for its cure. But, after all, *this* is not to be wondered at, for with such a meagre pathology of the disorder as we possess, what can be expected other than that which has taken place, a vulgar empiricism and the use of means as different, as distinctly opposite, as can possibly be thought or made use of.

Some appear to fix their attention on the prevention of a superabundance of fibrine, because they have been taught to believe that the blood contains too much of this constituent; but the grounds upon which such an opinion has been based appear to me to be altogether insufficient, for although it may be quite true as stated by Andral, that the blood examined by him in acute rheumatism held in solution as much as from $2\frac{1}{2}$ to 10 parts of fibrine in the 1,000, yet it must not be forgotten, indeed it should, I think, be particularly remembered, that this super-

fibrination might have depended largely, if not altogether, upon the immense quantity of blood taken by means of the lancet in his day, thus carrying off innumerable red particles, and with them so much of the watery constituents that when joined to the loss occasioned by profuse perspirations, there could scarcely be sufficient aqueous matter remaining to keep the fibrine at its proper level. I say, then, that if these things be taken into consideration, it will appear to be most probable that the cause of any apparent superfluity of fibrine was produced, not as a prominent or peculiar feature of the complaint, or that it in any way formed an essential part of it, but that it was the result of man's own proceeding on the one hand—the bleeding,—and nature's own effort on the other—the perspiration.

The late Dr. Todd, in the Croonian Lectures delivered at the Royal College of Physicians in 1843, calls into question this idea of the hyper-fibrinated condition of the blood, and whilst denying the right of the state of the joints to be considered as proceeding from a true inflammation, gives his opinion that in this affection there is a deficiency of the red corpuscles, and that therefore, in this respect, there is a resemblance to certain forms of anæmia.

Again, others seem under the conviction that to be of any use, the treatment employed must be addressed especially to the neutralization of the peccant acid which, as they believe, taints the

whole circulating current, and hence their "methodus medendi" consists in the administration internally, and the application externally, of potash in a variety of combinations. The presence of the noxious acid is to their minds made evident by the sour perspirations, and the intensely acid reaction of the urine, not forgetting the large deposit of urates which is so frequent an accompaniment of the complaint.

Dr. Inman, of Liverpool, who appears to have a thorough contempt for the alkaline treatment, states that the sour smell of the perspiration does not proceed from the presence of any acid whatever, but is solely the result of decomposition; and beyond this, he ignores altogether the notion that an acid of any sort is the poison occasioning the malady: he gives an acid, lime-juice, as his "sheet anchor."

But there are some of those who rely upon the alkaline treatment, who do so *not* from any belief that it does good by neutralizing an acid, but from the circumstance of its being able to prevent the occurrence of that which they seem to dread—an excess of fibrine; whilst there are yet others who advocate the same measure to be employed, not from any faith in its being able to influence the amount of fibrine present in so many parts of the fluid, but from its known capability of ensuring the continued *solubility* of this ingredient, and thus preventing a deposit of it under unfavourable circumstances and in undesirable localities.

Fortunate is it for the public of the present day that the faith of the profession is not now plighted, as in the days of Bouillaud and Chomel, to blood-letting as a panacea for rheumatic fever. One stands fairly aghast on reading the recommendation of the former of these two authors: it only serves to show to what extent even able men may be carried by any "hobby" they have fairly given themselves up to. As a warning against this and all other extremes, I am tempted to give the directions Bouillaud recommended to be followed in the treatment of the complaint upon which I am commenting.*

He commences his account of the treatment by saying "The true specific of acute articular rheumatism, its *quinine*, if the expression is allowable, is the antiphlogistic system, and bleeding is the prince of antiphlogistics." He then proceeds to give directions for the manner in which the bleedings are to be carried on, his mode being "*coup sur coup*." He next refers to the effect produced on the disorder, saying "the success obtained by this new formula of bleeding is such, that unless it was seen, it could not actually be credited.

* * * The average duration of acute rheumatism being only from one to two weeks instead

* Nouvelles Recherches sur le Rheumatisme articulaire aigu en général, et spécialement sur la Loi de Coïncidence de la Péricardite et de l'Endocardite avec cette Maladie, ainsi que sur l'Efficacité de la formule des Emissions sanguines coup sur coup dans son Traitement. Par J. Bouillaud, Prof. de Clin. Med. de Paris.—Paris, 1836.

of from six to eight." And then comes the pith of his method.

"*The day* on which the patient enters (supposing him to be of strong constitution and at a vigorous age) at the evening visit he is bled to sixteen ounces (for very sanguineous subjects this bleeding is sometimes carried to twenty or twenty-four ounces). *Second Day.*—Two bleedings of from fourteen to sixteen ounces each, and in the intervals of the two bleedings, a local bleeding, either by leeches or cupping (the latter is preferred). By this local bleeding, twelve, sixteen, or twenty ounces of blood are drawn. The cupping-glasses are applied around the most inflamed articulations and upon the præcordial region when the heart is seriously affected, that is to say, in the great majority of cases. *Third Day.*—A fourth bleeding similar to that of the evening of the previous day, and a second cupping (from twelve to sixteen ounces) either upon the præcordial region or around the articulations. *Fourth Day.*—The fever, the pains, the swelling, in a word, the whole inflammation sometimes ceases from the fourth day. In this case further bleeding is not performed, in the contrary case, another bleeding of twelve or sixteen ounces is performed. *Fifth Day.*—In general the resolution of the disease is in full activity, but in very serious cases the rheumatic fever may still be very marked, and twelve ounces of blood are drawn from the arm, or the same

quantity is taken locally. From the sixth, seventh, or eighth day, the convalescence is established, and nourishment may be commenced. * * * The medium quantity of blood taken from vigorous subjects, in a violent case, is from four to five pounds. In some cases it may be necessary to draw six, seven, or even eight pounds of blood."

And this was the practice of a really talented man and the Professor of Clinical Medicine "a la Faculté de Médecine de Paris." Can we feel surprised that the lancets of the profession are at this time permitted to rust at home for want of use, and that the public now lose the benefit that *might* be derived from a judicious use of the instrument?

Extremes are said sometimes to meet, but generally speaking they are wide apart. Dr. Gull proposes for acute rheumatism mint-water—plain, simple mint-water—which being interpreted, means, I suppose, that nature is to have her entire way in endeavouring to accomplish her own recovery. Yes! this is what it might be made to look like by simply saying "acute rheumatism treated with mint-water," but how different would it be found to be in point of fact?

I do not believe—I do not think that any other Physician would believe—Dr. Gull for one moment contemplates that he should neglect to pay attention to any and every complication that may arise during the course of the complaint; that he would not think it necessary in some instances to make

use of mustard plaisters, or poultices, or blisters, or leeches, or even the lancet; that he would be regardless of the temperature of the room, or the ventilation of it; that he would pay no attention to the intestinal or urinary secretions; that he would not order purgatives or diuretics if required; or that he would allow his patients suffering from acute rheumatism to drink new sweet strong Scotch ale, and eat mullagatawney soup and rump steaks with oyster sauce for supper, if they requested permission to do so. On the contrary, I *do* believe that all these and other points would receive Dr. Gull's best and gravest attention, and that whilst he would avoid all undue interference with the natural process of *elimination* going on, he would carefully and judiciously endeavour to support nature in her natural attempts to cure the malady, encouraging her when requisite with such assistance as the rational practice of medicine indicates, repressing or endeavouring to guide her when he found her efforts to be either too energetic or prone to take a direction likely to implicate the functions of important organs, or, in fact, to be in any way adverse to the one principal object it was desirable for her to accomplish: all this I am convinced he would do in proper cases. Is it then fair—can it be fair—to call such management, *Treatment by Mint-Water*? And if not fair to call it this, how obviously *unfair* it must be to say—*the disease is left to nature*!

It is pretty well understood that the great majority of the public (and I fear some few members of our profession) are fully impressed with the idea that anything that may be prescribed cannot be "medicine," unless it comes out of a glass bottle or mahogany drawer with a gold label and black letters upon it; and thus it is, that whilst the Physician's prescription is frequently kept most religiously locked up in a writing-desk, and shown only occasionally to admiring friends as proof of the good it either has done, or is expected to do, the advice and directions which accompanied it have passed "in at one ear and out at the other," never to be remembered again, although most probably of primary importance.

And so with regard to the treatment of the disorder in question—rheumatism. It might, with some, be considered that it is not treated with medicine, or in other words medically, "if left to nature," because no drug—no so-called physic is given as an *antidote* to the supposed poison. But the administration of an antidote implies an acquaintance with the specific poison, and as we are in no position to assert that we do know the nature of the poison, any attempt at neutralization must necessarily be a somewhat random proceeding. I hold it at the same time to be a fact that a disorder is treated medically not by *medicine* ALONE, but by ALL the et-ceteras comprised under the head of "general management," even supposing that no

drug of any kind be administered, and sometimes maladies are best treated so. This too may prove to be the case in acute rheumatism, but because faith cannot be placed in any one of the proposed "*specific*" means of treating the affection, I protest against its being left entirely to nature, and against the idea that it ever is so left, or is ever likely to be.

Whether the treatment by potash of Basham, the lime-juice of Rees, the blanketing of Chambers, the blistering of Davies, or the mint-water of Gull, be made use of, I take it for granted that not one of these authorities expects an adherence only and altogether to the one particular thing with which his name is associated, and still further, I am persuaded that in the treatment of acute rheumatism, it is as necessary as it is in any other ailment, not to forget whilst generalizing the disorder to individualize the patient.

ON BLISTERING IN ACUTE RHEUMATISM.

Although so many special remedies have been proposed for the treatment of acute rheumatism, and although, without doubt, they may each of them prove of service in properly selected cases, yet there is one that in my hands has never disappointed me in affording marked and almost immediate relief from those agonizing articular pains which, with very few exceptions, are found to be present

in acute rheumatic fever—and that remedy is the blistering method of Dr. Herbert Davies. Furthermore, I can fully endorse his opinion, that where this remedy is made use of sufficiently early, and before the appearance of any pericardial or endocardial symptoms are apparent, immunity from cardiac complications will be obtained.

The cases of acute rheumatism admitted into the Stafford General Infirmary are comparatively numerous, and indeed one may venture to say that this disorder is one of the most prevalent ailments (if really it be not the most prevalent ailment) of the neighbourhood. This circumstance has therefore afforded me the opportunity of giving the “blistering treatment” a tolerably fair trial, and I shall now present a short account of some few cases illustrative of what I believe to be the advantages resulting from the use of it.

I must, however, preface my account by plainly stating that none of *my* cases have been of such a character as would have allowed of my treating them by blistering *alone*, or at least I may venture to say, that had I done such a thing I should have been, in my own opinion, very culpable. In almost every instance there was at some time or other during the attack a plain and not to be mistaken indication for the administration of a cathartic or an aperient, a tonic or an alterative, a sedative or stimulant, but I by no means admit that the use of these, or any such remedies, detracts in any way

from the merit that may, I consider, be fairly claimed for the relief obtained from severe pain, and the immunity secured from cardiac complications, by the aid of the blister treatment of Dr. Davies.

CASE THE FIRST.

H— M—, aged 27, a wheelwright by trade, was admitted as an in-patient of the Stafford General Infirmary, on April 2nd, 1865. He had been ailing for some few days and had “flying pains” about his joints, with chilliness and a general feeling of indisposition. On the day of his admission he was feverish and very thirsty, whilst the pains in his joints had become much worse and his ankles and knees were swollen, but the wrists and elbows seemed free from swelling. His tongue was white and moist; his pulse 90 and rather soft; his bowels were open and the evacuations natural; urine extremely high coloured; specific gravity 1.029 and intensely acid; his respiration was natural; no pericardial or endocardial murmur could be heard. He shivered a good deal and complained of headache. I learned that his habits had not been temperate, and he had been on one or two occasions very ill, he said, with “bilious attacks.” He had never had rheumatism in any form before, and he was not aware of any of his relatives having suffered from the complaint.

He was ordered to have a warm bath, his joints

to be covered with cotton wool, and to take nitrate of potash, acetate of potash, and acetate of ammonia every four hours. He was put on a milk diet with a pint of beef tea daily.

Upon getting into bed after his warm bath, he began to perspire freely, and he passed a not very bad night. The knee and ankle joints were still swollen and painful, and he had had some "twinges" in his left elbow, but it had no swelling about it. On the following day he was scarcely so well, had passed a bad night, and had been a little delirious; pulse 87 and weak; his joints were acutely painful; perspiration moderate; he complained of feeling cold; the urine was unaltered; his bowels had not been acted upon since the previous day. I directed that he should have another warm bath; and each affected joint to have an inch and a half blister placed immediately above and around it. The medicine was to be continued and the diet was to remain unaltered; four ounces of sherry were to be given in the twenty-four hours.

Almost immediately after coming out of the warm bath he began to perspire very freely, and directly after the application of the blisters he went to sleep, and continued to sleep for nearly six hours.

On the morning of the 5th he said "that all his rheumatism had gone." His tongue was much cleaner; pulse 84, soft, and tolerably full. He looked lively and spoke cheerfully, declaring him-

self to be "quite a different man." His urine contained a large quantity of lithates, and had a specific gravity of 1.025; its chemical reaction was very faintly alkaline.

His improvement continued in an uninterrupted manner for ten days, when he had some slight return of pain and swelling in the left knee. This was again blistered, and with the same good result as before. He was discharged "cured" on May the 4th, having been in the Infirmary four weeks and four days. He had not at any time during his treatment the least symptom of heart complication.

CASE THE SECOND.

W— S—, an agricultural labourer, aged 60, was admitted as an in-patient on May the 19th, 1865. He was a man with a healthy, ruddy-looking complexion, and but for repeated rheumatic attacks his general health appeared to have been very good. This was the fifth time, he said, that he had suffered from rheumatic fever, the first time being when he was in his thirty-ninth year. This attack was not only the first but the most severe he had ever had and lasted nearly three months. He had been "leeches" and "blistered" over the region of the heart, and had been bled once from the arm.

On admission to the Infirmary he was in a very helpless condition; his ankles, knees, wrists, and

elbows being all swollen and very painful; his tongue was moist and white; pulse 100, and vibrating under the finger; his urine high-coloured and depositing large quantities of lithates; its specific gravity 1.022, with an acid reaction. On listening to the heart a very loud endocardial murmur was heard, accompanying the first sound and heard plainest at the base of the organ. He told me that he had been troubled with palpitations ever since his first rheumatic illness, and that sometimes when at work he "went very giddy." His febrile symptoms were very moderate; he had but little thirst and scarcely any appetite; the perspiration was moderate, but extremely sour; the bowels were constipated.

He had been ill for five days before coming to the Infirmary, and having had little or no sleep, begged that he might have something given him to procure rest.

He was ordered to have a warm bath at bedtime and to take the following medicine:—two-thirds of a grain of hydrochlorate of morphia at night, and half an ounce of castor oil the next morning; five grains of iodide of potassium three times a day, and two grains of quinine twice a day; his diet to consist of beef tea and milk, with three ounces of sherry daily.

On seeing him the next day I enquired if he had slept, he replied "that he did not know, for he had been rambling about his *work* all night long."

His joints, he said, were more painful than they had been; I therefore directed the blisters to be applied; this was done with the effect of procuring for him a thoroughly good night; the pains in his joints had gone, and the following day he expressed himself as being in every way better, his pulse was slower, 80, but possessing the same vibrating character; tongue cleaner; urine clear, of specific gravity 1.017, faintly acid and very slightly albuminous. The respiration was 15 in the minute, the endocardial murmur in "statu quo." He continued free from rheumatic pains, and left the Institution "cured" on June the 22nd, having been there four weeks and six days. The cardiac affection was unaltered.

CASE THE THIRD.

M—C—, aged 13, was admitted as an in-patient at the Stafford General Infirmary, on October the 24th, 1865. She was a good-looking, well nourished child, of dark complexion, and her general appearance was indicative of a vigorous constitution. She had suffered from acute rheumatism about eighteen months previously, but beyond the information that she had *not* been "very ill" I could get no satisfactory account of anything connected with the attack in question.

Her parents were very poor people, her father being a shoemaker: he for some years had been a martyr to chronic rheumatism.

The child, on admission, was in great pain; the joints of the knees and elbows, but particularly the former, were much swollen and the least movement of any of her limbs seem to occasion the most acute suffering; the face was flushed; the tongue "creamy white" and moist; the pulse 108 and hard. She perspired very freely, the perspiration being accompanied with the characteristic odour. She was thirsty, complained of head-ache, and could not sleep; the urine was high coloured, clear, and very acid; the specific gravity 1.022. Her bowels were constipated, but had been acted upon by a dose of castor oil on the day of her admission.

Her respiration was laboured, 30 in the minute, and on examining the condition of the heart a very distinct pericardial "to-and-fro" sound was heard, but there was no endocardial murmur, both systole and diastole were perfectly free and clear from any "*bruit*." Her illness had commenced about a week prior to her admission.

I directed a blister to be applied over the præcordial region, the affected joints to be wrapped in cotton wool, and the following medicine to be given:—Nitrate of potash, acetate of potash, and acetate of ammonia, every four hours, with an ounce of guaiacum mixture night and morning: her diet to consist principally of milk.

On the 25th and 26th she appeared to be much in the same state. The breathing was scarcely so hurried, 22 in the minute; the joints were more

swollen than they had been and quite as painful; she had had little or no sleep; her pulse 100. A blister an inch and a half wide was directed to be applied so as to encircle each limb immediately above, but not upon the affected joints. This was carried out soon after eleven a.m. on the 27th.

On seeing her the following morning I found a very visible and gratifying improvement. She was smiling, and to all appearance quite out of pain. The blisters had risen well, and she had had a very good night's rest in spite of the smarting occasioned by the blisters; the tongue was cleaner at the edges; she was much less flushed in the face; and her head-ache had quite gone; her thirst was considerably diminished, and she perspired much less than she had done; her pulse was 86. The urine was now alkaline and somewhat high-coloured. The specific gravity 1.018.

The pericardial "to-and-fro" sound could be scarcely heard, and the respiration was no longer so hurried, 16 in the minute. From this time she steadily improved and left the Infirmary perfectly well on November the 23rd, having been just four weeks in the house. She had *no* detectable symptom of heart mischief of any kind at the time she left.

CASE THE FOURTH.

J— L—, aged 38, a pensioner, was admitted on June the 30th, 1865. He had been in a hot

climate, and had had, he said, three or four attacks of "Jungle Fever," but had never had rheumatism before. Various members of his family, he informed me, had been very subject to rheumatism.

He was a well made man, but somewhat emaciated; the pulse was 110, soft and full; the urine of specific gravity 1.020, was very acid and high-coloured but clear: the tongue moist and yellowish-white in colour; the sounds of the heart were clear and distinct; there was some amount of hepatic tenderness, and the liver extended beyond its natural limits, the edges of the organ feeling rounded; the bowels were freely open, the evacuations being of a light clay colour, he suffered a good deal at times from hæmorrhoids—the respirations were 17 in the minute. He was perspiring very freely, was very thirsty and had no appetite; his knees and elbows were the joints principally affected: they were enormously swelled and so exquisitely painful that the least movement occasioned suffering so agonizing that he fairly shouted with it. He had only been ill two days.

His knee, elbow, and left wrist joints were directed to be blistered as in the previous cases, and he was directed to take two grains of blue pill with eight grains of compound ipecacuanha powder every six hours: he was put upon milk diet.

The following morning he was much better; he said he had pain in his joints, but not the same kind of pain that he had before the blistering, in

fact, he did not seem to be very certain whether his pain was the result of the blisters or the rheumatism; his tongue was a little, not much, cleaner; his pulse 92. The urine was plentiful, but still high-coloured and without any sediment, specific gravity 1.018; it had a very slightly acid re-action. The perspiration was less profuse; the bowels had not been acted upon; he had had some sleep during the night, and felt, he said, disposed to go to sleep again. The blisters had taken great effect, and the discharge from them was considerable.

The diet and medicine were ordered to be continued, and the blistered surfaces to have linseed-meal poultices applied to them.

On July the 2nd he was in all respects much improved, the pain in the joints was gone, but there was considerable stiffness from the blisters, his tongue was nearly clean, his pulse 84, his urine deposited lithates very freely, it was still high-coloured, the specific gravity 1.020, and the re-action neutral;—he was not nearly so thirsty, and asked to be permitted to have some solid food; his bowels had only been acted upon once, the evacuation being firm and clay-coloured, but not so offensive in odour as on his admission.

He continued improving for the following week, at the expiration of which his right wrist became a little painful and swelled, and his bowels were rather obstinately constipated. He had ceased for some days taking the blue pill and ipecacuanha

powder: I ordered him a dose of castor oil, which acted very freely, and the following morning the pain and swelling of the wrist had totally gone. He now had no further drawback, and left the Infirmary "cured" on July the 20th, having been under treatment just three weeks.

CASE THE FIFTH.

S— P—, aged 30, an agricultural labourer, was admitted on June 2nd. He was suffering for the first time from rheumatic fever, and had been ill three or four days before coming to the Infirmary. He did not appear to be possessed of a very robust constitution, and his outward appearance was not indicative of very great bodily strength.

The account I got of his "antecedents" was not unfavourable. He had lived all his life in the country, and been always more or less engaged in agricultural labour; his habits had been thoroughly temperate; neither his father nor mother had ever had any rheumatic attack that he was aware of.

His symptoms on admission were as follows:—Tongue moist and "creamy white;" pulse 108; urine high-coloured, clear, rather scanty in quantity, acid, and of specific gravity 1.020; the bowels were very constipated; the perspiration profuse; and the heart sounds quite natural. He was very thirsty, and complained of head-ache. He said all his joints were (and indeed all his body was) so pain-

ful he could scarcely bear to be touched or moved. His knees and elbows were much swollen: all his other joints were free from swelling; he had not slept for the last two nights, but had got some "snatches of sleep" during the day.

He had three grains of calomel, and ten grains of compound extract of colocynth given to him immediately, and the same kind of mixture of nitrate and acetate of potash that I had prescribed in the previous cases was ordered to be taken, and blisters were directed to be applied round the knees and elbows. His diet was to be milk and beef tea.

I saw him on the following day; he had passed a tolerably comfortable night, and expressed himself as being much better—the pains of his joints less, and the general sensation of soreness quite gone; but he was sensible every now and then of a smart twinge of pain first in one ankle joint and then in the other. The right ankle was swollen, the left was not swollen. I directed the blisters to be applied to the two ankles, and also to be applied again to the right knee, as the blistering fluid had produced scarcely any effect on the skin of this joint. The pills had acted well, his tongue was still white, and he continued to perspire freely; the urine remained high-coloured, but was more in quantity, clear, acid, and of specific gravity 1.019.

I did not see him for a few days, and on my next visit learned that his urine had been albu-

minous, and that he had had some slight "stranguary;" both these conditions had, however, disappeared, and he was in every way improved. The rheumatic pains were all gone, but the blistered surfaces were very sore; his tongue was clean; his pulse 82; the urine was lighter in colour and deposited lithates very freely, the specific gravity was 1.018.

He had not a single bad symptom about him, and was discharged cured on June the 22nd, having been in the Institution twenty days.

CASE THE SIXTH.

E— T—, aged 24, labourer, was admitted on September the 8th, 1865. This was also a first attack of rheumatism, and he had been ill for ten days before I saw him; he had not previously had very good health, and about three years ago had had a long illness from "fever," but of what precise kind I could not learn. All his relatives had been, as far as he knew, free from rheumatism.

He was now in a highly feverish condition, the arthritic pains were of a very fugitive character, attacking sometimes one joint and sometimes another, but all his joints, that is to say his knees, ankles, wrists, and elbows, were more or less swelled, and when admitted his knees were exquisitely painful; the other articulations, however, were by no means in so bad a condition—his tongue

was moist and not very white in appearance; his urine rather high-coloured, acid, and depositing lithates in large quantities; his bowels were constipated; his thirst was moderate; his pulse 90, full and soft; there was no evidence of any heart affection; he was very restless, and complained much of want of sleep.

He was ordered a cathartic, and to have the blisters applied to each affected articulation; he was also directed to take the potash mixture and to have a milk diet.

On the 10th he passed a comparatively comfortable night, and said he should have "slept better if it had not been for the soreness of the blisters," admitting, nevertheless, that he would prefer the soreness of the blisters "twenty times over" to the pain he had suffered from the rheumatism.

His pulse was 86; his urine neutral and of specific gravity 1.023, still depositing lithates; his bowels had been acted upon, but not very freely; his tongue was clean; his appetite much improved, and although he perspired the perspired fluid was not excessive.

The treatment was continued and the following addition was made to it—he was to take night and morning an ounce and a half of guiacum mixture until his bowels were sufficiently acted upon. His improvement continued in an uninterrupted manner, and he left the Infirmary "cured" on June the 21st, having been under treatment for thirteen days.

COMMENTS.

I could, without difficulty, give pretty nearly a score of similar cases, in all of which the blistering proved of as marked and unmistakeable service as in those I have just described; but as it would be tedious to do this, I shall satisfy myself, and I hope my readers, by saying that after the proof I have had, I cannot hesitate to express my belief that this method will be found a most valuable adjuvant in the treatment of a most troublesome and, in the generality of instances, a most painful disorder.

In every case in which I have adopted it, the relief from rheumatic pain has been beyond all doubt immediate, and for the most part permanent, and in no case have I had the supervention of pericardial or endocardial inflammation after the application of the blisters—so that, as far as I can judge, it seems to endow the patient with a sort of immunity from these complications.

It will be observed that my cases were none of them treated with blisters *alone*, and after what I have said upon “special remedies,” it could not be expected that I should do so, for although thoroughly convinced in my own mind that Dr. Davies’ treatment relieves pain and prevents cardiac complications, I am equally convinced that every case must be, or at any rate should be, treated

on its own merits. Each case may require some modification in its management, and *special remedies* must all be used to some extent in an empirical manner until the *special pathology* of rheumatism is better understood; and as Dr. Fuller has observed in his work on rheumatism, "what we want is far less the discovery of any new medicines, than the adoption of our present remedies to the exigencies of each case."

In concluding these remarks I may add, that in the most extensive blisterings in this malady I have met with only one instance of stranguary being produced, and this was very trifling in its nature and rapidly passed off; but I have in some few instances found the blistered surfaces difficult to heal, and indeed this circumstance has not unfrequently been the occasion of the patient being longer under treatment than would have been otherwise necessary, the rheumatism having quite gone, but the blistered surfaces remaining unhealed. In a Metropolitan Hospital, from want of room to meet the many urgent cases which present themselves, of course such patients would be discharged, or continued as out-patients; but in a Provincial Hospital, the smaller number of patients allows of these cases being detained until their entire condition is such as to admit of their at once resuming their usual occupations.

I do not think it is, at present, possible to give any satisfactory opinion as to how the blistering

treatment produces its beneficial effects—the defective pathology of the malady is a sufficient debarment to any explanation of the *modus operandi*—but I may say that I do not think any particular acid is got rid of by means of the serum discharged, as all my attempts to detect anything of the kind have proved fruitless; in every case the serum has been either alkaline or neutral.

The effect upon the urine is nevertheless very decided, for however acid this secretion may have been before the blisters have been applied, it very speedily becomes, after their application, either alkaline or neutral, and this, too, in cases treated without the administration of any alkalies whatever.

HISTORY V.

RHEUMATIC FEVER WITHOUT PAIN.

Is it not generally the case with most, if not all of us, that we use the terms "disorder" and "disease" as synonymous expressions? And, if so—are we correct in so doing? To me it appears that the term "disorder" would most properly be restricted to mere functional disturbance, and "disease" to organic alteration of ultimate structure. It is evident that we may have functional disorder without organic "disease," but there cannot be the latter without the former. Functional disorder *alone* admits, I think I may say almost universally of an absolute cure; whilst in organic alteration of ultimate structure (*i.e.* "disease") we can only hope to benefit by adopting accommodating and palliative measures. Such a division might, I imagine, be found serviceable in classifying maladies under the two heads, curable—"disorders"—and incurable—"diseases."—In the former list I conclude we should be justified in placing recent and first attacks of acute rheumatism.

From nearly the earliest periods of medicine to the present, the cause, or as it is customary to call

it, the *materies morbi* of this affection has been eagerly and anxiously sought for. That it is specific in its character I think there can be no doubt whatever, any more than there can be as to the specific nature of the blood poison in many other fevers. But there our knowledge stops, for I must believe we are as far now as we have ever been from possessing any exact information as to the absolute cause of the disorder.

The chemical theory—the presence of lactic acid; the hyperinosis theory, and other theories, have each and all had their clever and able advocates; and now we have some very recent observations brought forward for our consideration touching the origin of this ailment which, while startling from their novelty, are likely to prove largely subversive of much that has hitherto been surmised, or speculated upon, as to the true *origo et fons mali*.

In a work which I have before referred to (in the latter part of the history of two cases of cerebro-spinal meningitis, and again in the history of acute rheumatism treated by blisters), written by Doctors Mitchell, Moorhouse, and Keen, there are the following valuable remarks on the alteration in the nutrition of joints occasioned by nerve injuries.

“Again we call attention to a peculiarity of nerve injuries, which we believe to have been overlooked.

“Like the altered nutrition of the skin, the symptom which we are at present considering occurs at any time after the few first days. It consists essentially in a painful swelling of the joints, which may attack any or all of the articulations of a member. It is distinct from the early swelling due to the inflammation about the wound itself, although it may be masked by it for a time, nor is it merely a part of the general œdema which is a common consequence of wounds; it is more than these, more important, more persistent—once fully established it keeps the joints stiff and sore for weeks or months.

“When the acute stage has departed the tissues about the articulations become hard, and partial ankylosis results, so that in many cases the only final cause of loss of motion is due to this state of the joints. Of all the agencies which impede movement it is the most difficult to relieve.

“Were we asked to state in what essential respect these lesions differ from sub-acute rheumatic disease of the same parts, we should certainly be at some loss to discern a difference.

“The subject suggests certain interesting reflections. We have ourselves seen cases of spinal injury in which rheumatic symptoms seemed to have been among the consequences, and four such instances of striking character are to be found recorded in a paper by the late Professor J. K. Mitchell, *American Journal of the Medical Sciences*,

vol. 8, p. 55. Upon the hints which were thus furnished, Dr. Mitchell was induced to consider rheumatism as of spinal origin.

"His treatment, founded on this view, was most successful, and is still used and recognised in this country. Modern pathologists have traced the causation of rheumatism to a strictly chemical source, but no one can avoid seeing that there may be a cause beyond this, even though the chemical conditions be still considered as essential links in the chain.

"Thus, after all, the *true origin* may be spinal, or, at all events, the indisputable fact that there are rheumatisms depending for existence on neural changes, may aid us hereafter to discriminate varieties of type among the forms of rheumatic diseases.

"It were easy to dwell upon this subject, but enough has been said to show that sub-acute inflammations of joints may be brought about by nerve lesions, and to direct medical thought anew in a direction which seems favourable to its true and rational progress."

In the cases referred to by these gentlemen, it was quite plain that the *supposed* necessary order of events was reversed; the nerve irritation was not provoked by any previous rheumatic condition, but to all intents and purposes the rheumatic attack was dependent upon nerve lesion.

We have also additional evidence to show that

neither the sequences nor consequences of this disorder follow in one uniform order any more than is found to be the case with a host of other ailments.

Dr. Sutton, in the *Medical Times and Gazette* for July 2, 1864, relates a case of acute rheumatism, in which the pericarditis preceded the usual rheumatic symptoms by five days, and even then the indications of the malady were so very slight, and lasted for such a brief period that, he says, they might very easily have been overlooked, there being really and indeed neither pain, swelling, nor tenderness on pressure of any joint.

But many other writers, including Chomel and Graves, have commented upon the existence of acute rheumatism without the presence of any arthritic symptoms whatever; so that, as the American writers I have just quoted say, "There may be a cause *beyond* the chemical one, even though the chemical conditions be still considered essential links in the chain."

That there should be a difference or differences of type in this affection is only what might be naturally expected, for it is an incontrovertible fact, that one specimen of the genus "Homo" varies as much from another in point of constitutional qualities as in outward or personal appearance, and consequently for any very great number of persons to be affected precisely alike—the symptoms and their order of appearance being in every instance the

same—must be an occurrence of comparative rarity.

Acute rheumatism *without pain*, sounds, I dare say, oddly enough; but, not more so I imagine than does scarlet fever or measles without an eruption. That the former as well as the latter may happen is now very generally admitted, and I give the following history as a very good example of it:—

CASE.

On the morning of the 5th of December, 1864, I was sent for by Mr. J— G—, aged 36, a farmer, in very comfortable circumstance, residing about six miles from Stafford. He was of a spare habit of body, about five feet ten inches in height, somewhat swarthy in complexion and bilious in point of temperament: without ever having had any severe illness (excepting that two years previously he suffered from a pretty sharp attack of diphtheria), he was never in anything like vigorous health.

His method of living was regular and steady, but being very fond of “field sports,” he generally indulged in them freely. The day before I saw him he had been out with the hounds. After a tolerably smart gallop, which made him perspire freely, the riders came to a check by the side of a “cover,” and whilst the hounds were in “cover” my patient was waiting outside, the wind blowing

bitterly from the north-east. This, he said, chilled him very much, and when he got home he felt so unwell that he went to bed. He slept moderately well, but somewhat uncomfortably.

On attempting to get out of bed in the morning, he found that he had difficulty in moving his legs, and when he placed his feet on the floor he could not stand. He now observed that his ankles and knees were much swollen, and his left wrist and elbow were in a similar condition, but the movement of his arm was not interfered with. He had, however, *no pain in any of his joints*, but felt, as he expressed himself, low, irritable and out of spirits. He was a good deal alarmed at his state and thinking that he was suffering from paralysis sent direct off for me.

On visiting him, I found his condition to be as before described, but I also observed that the articulations, although swollen, were neither hot nor painful when pressed upon. His tongue was coated with a whitish sort of fur; his pulse was 86, but very deliberate, and hard under the finger. The urine was high-coloured, and had a specific gravity of 1.022, with a strongly acid reaction. I was told that the bowels acted regularly once or twice in the day.

He did not complain of pain anywhere, but said he had a sensation of soreness all down the centre of his back. Upon percussing over the spine, which I did throughout its whole length, he told

me that it communicated an uncomfortable feeling, but that he could not say the process was absolutely a painful one. The chest sounds, afforded both by percussion and auscultation, had nothing whatever about them to attract attention, excepting that, perhaps, the general respiratory murmur was weak; inspiration and expiration were quite natural and sixteen in the minute. The heart sounds were also weak; the first and second sounds, however, were perfectly distinct and clear, and the rythm was properly maintained.

He showed no disposition to perspire; in fact, he complained of being cold, and the temperature of the surface of his body was far from being very warm. The sense of touch in all his limbs was quite unimpaired.

I could not learn that he had ever had a rheumatic attack upon any previous occasion, and he was not aware that any of his relatives had been subject to the disorder.

I directed that he should have a warm bath; after it all his joints that were affected were to be well wrapped in cotton wool, and the sheets were to be removed from the bed so that he might lie between the blankets. I prescribed for him nitrate and acetate of potash with full doses of the acetate of ammonia, and an ounce and a half of guiacum mixture to be taken night and morning. His diet was to consist of milk and beef tea.

On visiting him the following day I was in-

formed that he had not slept well, and that after the warm bath, on getting into bed again, he broke out into a most profuse perspiration and was still in this condition, the odour being so excessively sour that it was complained of not only by his wife and the servant, but even by himself. His pulse had become quicker—96—and softer; the tongue was more furred and yellow in colour. He said he was now as much too hot as he had been too cold the day before, and that he was very thirsty; he also complained of head-ache. The urine was more high-coloured, scarcely so acid, and with a specific gravity of 1.026.

He was still continuing free from any pain in the joints, but admitted that they felt "stiff." The left wrist and elbow were scarcely so swelled, but the right wrist and elbow were now affected and larger than the left. He was quite helpless and unable to move by any effort of his own. There was a little more cheerfulness about him, but he was still very depressed in spirits; the bowels had not been moved, and the conjunctival surface was assuming a yellowish tinge. He was ordered to continue the treatment, and in addition to have a cathartic of calomel and compound extract of colocynth, immediately.

I saw him again on the following day and found him to be, in some respects, better; the tongue was cleaner, especially at the edges; the bowels had been freely acted on, and the swelling of the left

wrist and elbow was very much reduced. All the other joints were quite as much swollen as they had previously been, but still he had no actual pain in them. With regard to the left arm he said that although smaller it felt "stiffer" than any of his other limbs. He accounted for this uncomfortable sensation in it by his having, in his sleep, got it from under the bed-clothes, and by the cotton wool having become displaced.

I examined his spine again, but he bore percussion without the least flinching or sign of pain. His thirst was not so great, but he fairly steamed with perspiration, which was as sour in odour as it well could be; he was in much better spirits, and I requested that the entire treatment might be persevered with.

For the next few days there was but little change in the state of the patient, excepting that sometimes one joint and sometimes another would be a little more or less swollen than the day before. On the seventh day he complained of feeling cold and "shivery;" the articulations of both arms were less swelled than they had been, but nevertheless they were more uncomfortable. He also said that he had a sense of oppression in his breathing, and he felt uneasy about not perspiring so much as he had perspired.

On listening to the heart there was heard a very gentle pericardial "to-and-fro" sound, but no endocardial "*bruit*" of any kind. I thought the

systole and diastole a little more vigorously performed than before, but nothing beyond this; the pulse was 100, rather thin and hard. He had scarcely slept at all during the night, but he could not account for this in any other way than that he felt restless and cold, and that his breathing was not easy. The urine was still high-coloured, and acid; the specific gravity 1.022. His bowels were acting freely. His thirst was about the same and his tongue cleaner.

He was directed to have repeated mustard plaster over the region of the heart, and a warm bath of the temperature of 98°. He was to continue the same diet, that is milk and beef tea, and to have four ounces of sherry in the twelve hours. The saline and guaiacum mixtures were to be stopped and to be replaced by five grains of the iodide of potassium in an ounce of distilled water every four hours, and two grains of disulphate of quinine twice a day.

In consequence of the pericardial friction sound, I saw him early on the following morning. He had had a very much better night, having slept well for several consecutive hours. He was feeling warm again, and perspiring as freely as ever. His joints were more swollen, but not so stiff and there was no pain in them. I could hear nothing whatever of the "to-and-fro" sound. The pulse was 90 and soft; the tongue very much cleaner; the breathing considerably easier but not quite so free as usual. The urine

was acid, specific gravity 1.028, and loaded with lithates, indeed fairly thick with them. The bowels had been acted upon twice since my last visit; the thirst was much diminished, and for the first time since he was attacked he expressed himself as feeling hungry.

From this day his progress was satisfactory, although somewhat slow; the joints gradually came down to their proper size, but continued very stiff. He was nevertheless well enough to join the family party on Christmas-Day.

A few days afterwards he most injudiciously exposed himself to some very bad weather, which occasioned a relapse. He passed through the same stages of the disorder as in the first instance, and his recovery was a little more protracted, but neither in the first nor second illness did he have any painful state of the joints, such as is usually seen in an attack of acute rheumatic fever. Stiffness of the joints he had both at the time and for some considerable period after he became convalescent. In fact he never quite lost this feeling until the summer, when he went to Buxton and had some baths; these seemed to remove it effectually.

COMMENTS.

I fear we have all of us been so thoroughly content for years past, certainly from the time that Dr. Prout put forth his lactic acid theory, to accept the

chemical solution of the difficulty (*i. e.* the cause) of acute rheumatism, that there is naturally a great impediment to our adopting the belief of there being any other view. Indeed should any one now venture to propound the idea that there is some different condition upon which the entire phenomena of this affection rests, it would be thought, to say the most as well as the least that could be said of such a step, "*a bold proceeding.*" But fortunately for medicine, as well as for other studies, the old saying of "*so many men, so many minds,*" is quite as applicable now as ever it was, and thus the necessity or prudence of always keeping in one groove is liable to be called in question.

The case of Mr. J— G— had about it some few things which require a brief notice. In the first place, it is by no means a common occurrence for so severe an attack to commence in so very sudden a manner. He was in his usual best health up to the time of being "*chilled*" by the cover side, and yet on the following morning his lower limbs were excessively swollen at the joints and all use of them quite gone. Then there was the entire absence of pain. No pressure of any ordinary kind, no movement of a passive nature, occasioned him any uncomfortable sensation beyond that of mere "*stiffness.*" Again, although he experienced that lowness of spirits and general depression which almost invariably attend a rheumatic attack; yet there was no heat of the surface

of the body nor any appearance of perspiration until after the warm bath, nor any thirst until after he began to perspire. At one time some of the first symptoms actually returned, viz., the shivering and general coldness, and with them some slight pericardial complication, but these all yielded readily to the means used. It appeared to me that my patient was well charged with the requisite element of the disorder, but that from feeble constitutional power there was no vigour for its elimination. Hence my reason for directing a stimulant to be given at the time when the first symptoms referred to re-appeared.

At the time I saw this case I was quite unacquainted with the views of the American authorities I have alluded to, otherwise I should have been tempted to direct my treatment especially to the condition of the spinal nerves, for, in this instance, the complaint made by my patient of the "soreness down the centre of the back" most probably was dependent on some abnormal condition of the nerve structure in this immediate locality. I must admit that neither before nor since the occurrence of the case in question have I ever had *my attention called by any patient* suffering from this disorder, to a painful condition of the spine. But subsequently to reading the American work which treats of the effect of spinal nerve lesion on the nutrition of joints, I have in every case of acute rheumatism carefully examined the condition of the

spine, and certainly in the majority of cases, I have never failed in detecting some amount of spinal irritation. Sometimes a tolerably strong pressure has been sufficient to afford evidence of present tenderness, whilst in other cases a sharp percussion has been required to elicit the necessary proof.

Looking at "rheumatism" as one of those affections believed to be constitutional, and which like "gout" is accepted both by medical men and the public as an "*inevitable*," we cannot help feeling something more than ordinary anxiety for the discovery of its cause; and if anything could have the effect of further increasing the desire for such a consummation, it might, perhaps, be found in the circumstance that people generally are disposed to "*twit*" the profession, "*en masse*," not only with being unable either to prevent or cure the disorder, but also with the statement that the "nostrum" of the "empiric" is often equally as efficacious as the "specific" of the "physician."

Before concluding this subject I wish to express a belief that a close attention to the state of the spinal nerves in both acute and sub-acute rheumatism, coupled with carefully conducted experimental enquiry, would tend, ere long, to show that this very common and very intractable malady is infinitely more dependent on primary changes in the nervous system than has ever hitherto been imagined by any excepting our American medical brethren.

HISTORY VI.

CHOREA FROM SPINAL IRRITATION.

Amongst the various and varied kinds of obscure nervous disorders with which the human family is liable to be attacked, there are few that have given rise to more discussion, to greater diversity of opinion as to their cause, or to wider opposites of treatment than that by no means uncommon complaint *Chorea*. That this disorder should be so usually amenable to treatment, although so prone to relapse, is a circumstance which none can fail to rejoice at; but unfortunately, cases of it every now and then occur in which all efforts for its removal end in a failure, the sufferers continuing to be tormented with it during the remainder of, perhaps, a long life; whilst in other instances it proceeds to a rapidly, yes, a rapidly fatal termination. Can, then, all cases of chorea be dependent on one universal and unalterable cause, or must we with this affection, as with epilepsy, look for a variety

of causes as being capable, under certain circumstances, of producing a train of outward symptoms in almost every respect identical? There can be, I conceive, but one answer to this question.

In the German periodical, "Wochenschrift für die gesammte Heilkunde," No. 1, 1837, there was published a very able and, indeed, elaborate article on chorea, the author being Dr. Stiebel, who had had at that time the opportunity of seeing one hundred cases of this disease. From the opportunities thereby afforded him and from the observations he had made, he formed the opinion, and laboured hard to prove to others, that this affection was one *entirely* occasioned by irritation of the spinal nerves, this irritation being produced by turgescence of the vessels of the membranes covering either the spinal marrow or the medulla oblongata. But, whilst Dr. Stiebel affirmed that in every case he had examined, he could find *no other* cause than that above mentioned, yet it is right to add he did not absolutely deny, nay, he even distinctly admitted the *possibility* of other producing causes; and he gave as examples "injuries to the spine and metastasis of rheumatic inflammation:" but how these can act otherwise than by creating irritation of the spinal nerves—in other words, what these *are* but actually the very conditions which he considered as being essential to the production of the disorder, I cannot very clearly understand.

That chorea may be, or rather that a tendency to it may be, transmitted in an hereditary manner is, I imagine, beyond all manner of doubt, and its more than ordinary frequency amongst the Jews might properly be used as an argument in favour of such a proclivity. That, however, it may proceed from, or perhaps it would be better to say, be directly connected with, acute heart disease, and this too totally irrespective of any hereditary predisposition or symptom of rheumatism, is endeavoured to be proved by Dr. G. Burrows in an interesting paper of his, which appeared in the *Medical Gazette*, May 26, 1843. Still further, that it may be associated with, and *possibly* be one of the consequences of, either a past or present rheumatic attack, is now an admitted fact, whilst so frequent and so intimate has this union proved to be, that the enquiry as to a patient having had rheumatism if affected with chorea, is seldom, I should think, answered with a negative.

The following case of "Chorea," connected both with the effects of a previous rheumatic attack and the actual and present existence of acute rheumatic fever, will serve to confirm the opinion as to the relation of the two affections, and in addition will also tend to corroborate, if it does not indeed altogether substantiate, the views of those who place their entire belief on spinal nerve irritation as the chief, in fact, the only cause of the disorder.

CASE.

H— B—, aged nine years, had suffered, two years previously to my seeing him, from acute rheumatic fever, and on the third day of it he showed symptoms of heart complication, but I could not get any explicit information as to whether or not, on recovering from his illness, there was any detectable mischief of the heart remaining. On the 25th of January, 1864, I was requested by my friend Mr. Hughes to meet him in consultation, the boy having had another attack of the same disorder, and which after lasting for four days became complicated with "Chorea." It was on the thirteenth day of his illness that I was sent for, and on entering the room I found the patient in the following condition. He was lying, or it would be more correct to say, he was *attempting* to lie, on the bed, the clothes of which were in the most confused and disordered state, whilst he, poor fellow, was spasmodically jerked and twitched in almost every conceivable manner, every limb, and apparently every muscle, being in continual motion; the countenance was flushed and anxious; the eyes were prominent and had an enquiring look, the general expression of the face being intelligent: in disposition, I was told, the boy was mild and affectionate; he replied calmly and appropriately to every question, although the incessant spasmodic

movements made the act of speaking one of considerable effort; his pulse was rapid, 104, and wiry; the tongue was furred and yellowish in appearance; he perspired profusely, and there was the well known sour odour so common in rheumatic cases; but there was this peculiarity about his perspiring—he was rarely, I believe I might say never, in a general perspiration; sometimes one half of his body would be covered with perspiratory secretion, whilst the corresponding half would be comparatively, indeed perfectly dry, sometimes a limb, or limbs, would be thus excluded from the otherwise general condition of moisture, and at others the trunk *alone* would be bathed in perspiration, so that, correctly speaking, he perspired in patches, but under all circumstances the secretion possessed the peculiar acid odour. His bowels were moderately acted upon, and the evacuations were of a healthy character; the urine was high-coloured, acid, the specific gravity was 1.025, and it deposited no sediment; he complained of pain in almost all his joints and some of them had been greatly swollen, but none of them were very much swollen when I saw him; when percussed down the spine he complained of pain throughout its whole length.

His treatment up to the time of my seeing him had consisted of salines, colchicum, and opium, with blisters over the region of the heart. Upon listening to the heart a very distinct double

friction sound was heard over the whole area of its site, and accompanying both the first and second sounds was one of the most harsh, grating, whizzing "*bruit*" I ever remember to have met with; the impulse was felt far beyond its natural limits, but notwithstanding all this, the respiration seemed but slightly hurried. What with the heart's tumultuous action and the never-stopping spasmodic movements both of body and limbs, there was but little chance of carrying out an auscultatory examination with anything like precision; but, nevertheless, sufficient was accomplished to render the condition of this organ altogether unmistakeable.

He was taking, and had taken throughout, nourishment in the shape of milk and beef-tea without any disinclination, in fact, almost with an appetite, but the vessels containing the food had to be conveyed to his mouth by a second person, his own efforts at accomplishing it being quite ineffectual. He had slept but little, and that by snatches few and far between, but even during sleep the spasms were only lessened, not arrested; he seemed terribly worn out, and as everything that had been tried to procure sleep and muscular quiet had failed, I proposed that which in more than one similar instance I had found to succeed, a cold ablution suddenly performed, with several people to rub him rapidly dry; this for a while accomplished the purpose, and he had some rest with *complete* cessation of the movements, but only

for a very brief period, at the expiration of which they returned with certainly equal if not increased severity, and in defiance of every means used. I do not think anything at all likely to benefit him was omitted, but he died on the 28th completely exhausted, though retaining his consciousness up to almost the moment of his death. I have before spoken of his being a very amiable boy, and during the time that I saw him he seemed to possess a remarkable degree of self-control as far as his emotions were concerned. The friends, with some little difficulty, assented to the performance of a *post mortem* examination, the results of which I will now describe.

POST MORTEM EXAMINATION.

Upon opening the chest the lungs were seen lying in their natural position, and presenting indications of some slight superficial congestion; the pericardium looked redder than it should have done and was evidently very much distended, but not regularly so, for in places it seemed, as it were, tied down; upon slitting it up, the whole upper and anterior portion of the heart was found covered with a thick, clear, fibrinous clot of great size; laterally the pericardium was adherent to the heart itself, whilst posteriorly there was another thick, but smaller, fibrinous clot, resembling in its general character that before spoken of as being in

front of the organ. Upon cutting through the walls of the heart they presented nothing particular in appearance, being of normal thickness, but both the *carneæ columnæ* and *chordæ tendineæ* were coated with lymph-like exudation, apparently of a recent nature, and the aortic valves were roughened with small vegetative looking growths, evidently the result of some previous endocardial inflammation; the right auricle contained a semi-opaque spiral-formed fibrinous clot; the other cavities were empty: the lungs were somewhat congested internally, but not much so: the stomach, liver, kidneys, and intestines were all perfectly healthy. There was nothing about the brain and its membranes to call for particular notice. When the spinal canal was laid open, there was noticed a considerably larger amount of fluid than is usually seen in this part, and the vessels of the membranes were turgid with blood, some of them being ruptured and a small portion of their contents extravasated; the coverings of the spinal nerves, and the nerves themselves, presented the following singular appearances:—just at the point where the nerves are seen to emerge through the intervertebral notches they seemed pinched, almost as if tied by a ligature, whilst at this spot their membranous covering was at least four times as thick as it normally should be, so that it appeared to form a complete stricture, whilst both before and behind this constriction there was ample

evidence of inflammatory action. The entire length of the spinal canal was not opened, for although the relatives of the boy consented without any great amount of opposition to the body being examined, yet there were so many restrictions placed upon what they termed the mutilation of it, that we could only get permission to open a portion of the spine, and this we did from about the last cervical to the first of the lumbar vertebræ.

The body presented no appearance of wasting, in fact, it was that of a well-nourished lad, so that his sixteen days' illness seemed to have had no injurious effect on his outward condition, but *under the skin both of the body and limbs there were large discoloured spots almost like those seen in some of the worst forms of scarlet fever*, and although the inspection was made the day after his death, and the weather was cold, decomposition was fast commencing.

COMMENTS.

In this case certainly the connection between rheumatism and chorea was made palpably plain, and the morbid state of the spinal nerves and their coverings was such as to quite justify the opinion that irritation of the cord was the cause of the long continued and fatal involuntary movements.

And here *again* we must, I am of opinion, have our thoughts irresistibly attracted to the observations made by those American gentlemen

whose writings I have had occasion, indeed have felt it to be my duty, so frequently to allude to: in the present instance it is not only to their remarks on the influence produced by spinal nerve injuries on the state of the articulations that one's mind naturally reverts, but also to the strange effects produced on the appearance of the skin by nerve lesions, and to which these authors particularly direct attention.

The precise relation which such states bear the one to the other—by this I mean the position in which they stand to one another as “cause” and “effect”—I suppose can scarcely *yet* be said to have been proved to a demonstration: but the association of these conditions has been so frequently observed, and is so evident and striking, that there is impressed on the knowledge of these facts a very important significance which I cannot help thinking will, at no distant period, culminate in placing the pathology of both “rheumatism” and “chorea” in a much more decided and satisfactory light than has heretofore been attained. Necessarily with this will occur some modification in the plans of treatment at present had recourse to, with the effect, it may be fairly hoped and believed, not only of conferring a benefit upon those who have the misfortune to suffer from these maladies, but of giving, at least, the appearance of uniformity to that which certainly is now a great incongruity if not absolute chaos.

So far back as 1839, Dr. Bright drew attention to various spasmodic affections being brought on, as he believed, by inflammation of the pericardium, and he considered that the phrenic nerve was the channel by which the irritation was communicated to the spinal cord.

The first case reported by him may be found in No. 4 (second series) of the Medico-Chirurgical Transactions, and as in many respects it exhibits a marked similitude to the case I have narrated, I cannot refrain from offering an outline of it.—A young man, seventeen years old, suffered from general, but not acute, rheumatic symptoms: when apparently getting something better he was suddenly seized with singular spasmodic twitchings and involuntary muscular movements, which very shortly began to resemble the most violent attack of “chorea.” In spite of every means made use of the chorea became worse, some little wandering of the mind took place, and about the sixteenth day from the time he was affected he died exhausted. The day after he died a *post mortem* was made—the heart, pericardium, and contiguous portions of the lungs were said to be exclusively the seat of disease; the heart was adherent to the free pericardium, and there was the most profuse effusion of firm semi-transparent gelatinous fibrine. Dr. Bright goes on to say that the *brain* and *abdominal* viscera were most carefully and minutely examined, and all were perfectly

healthy. It must, however, be noticed that not one word is said about the *spinal cord* and its nerves, and therefore one is led to infer that they were not examined: had they been I have no doubt they would have been discovered to be in some similar state to that observed in the case of H— B—.

The second case related by Dr. Bright has many points of resemblance to the first, but when giving an account of the *post mortem*, it is explicitly stated that the head and spine were NOT inspected. It must not, however, be supposed that Dr. Bright ignored the connection between chorea and rheumatism, for he expresses his belief that the instances of it are numerous; nor that he did not think it probable the membranes of the cerebro-spinal mass might be implicated, but what he did was to express his opinion that it was the *pericardial* mischief communicated thence by the phrenic nerve to the spine which occasioned, or determined, the attack of chorea;—just as, he says, “the irritation of other parts, as of the bowels, the gums, or the uterus, is communicated and produces the same disease.” However this may be, certainly, the non-examination of the spinal nerves looks as if he had been more anxious to find a cause elsewhere rather than in the most likely neighbourhood.

Now, between those cases of chorea which are so numerous and so easily arrested, and those which are so refractory and fatal, there must

of necessity be either the operation of some different cause, or the cause, if the same, must be modified by some peculiar conditions. To my mind the difference consists in this, that in the milder and more yielding forms there is mere functional disturbance, the result most probably of sympathy without any active inflammatory action; while in the graver attacks there is *always* going on a local lesion of a severe character. True, the former might possibly run on into the latter, but my experience would almost lead me to say it rarely does. There has always been in the cases I have seen a very wide difference in the *kind* of spasmodic action in the two forms I refer to. In the milder and sympathetic state (I mean that of functional disturbance alone) the movements, although certainly spasmodic, have been *grotesque* in their manner, giving one a sort of feeling that they were rather exaggerated natural motions, with a shade of *controlability*, if I may use such a word; whilst in the severer forms all control is evidently gone, and the spasmodic affection seems to partake almost of the nature of tetanus.

Dr. Stiebel, the author I have before referred to, has, what he calls, an anatomical cause to assign as the producer of spinal irritation. In the milder forms of the disorder he considers that, as these cases are usually observed amongst young growing persons, it is probable that as the spinal marrow and the origin of its nerves lie within a bony canal,

there may be, during development, some want of due relation between the bones and the enclosed part of the nervous system, the cavity not corresponding to the increasing marrow, and thus a constriction or pressure may, for a time, produce an irritating effect: or, he says, this disproportion of parts may be the result of swelling of the spine without previous change in the nerves; but he thinks the former cause the more frequent.

TREATMENT.

As to treatment, the sympathetic, or mere functionally disordered form of chorea is, I consider, most successfully treated by attention to the general health, by a generous diet, metallic tonics such as zinc and iron, especially the former, and the daily use of the shower bath. Where chorea depends upon active local irritation of the spinal nerves, the result of either past or present inflammatory action—as for example in the case of H—B—, the effect of treatment will be found, I fear, to be of a very unsatisfactory character: some patients will live distressing and pitiable objects, and others will die with equal or greater rapidity than did the boy whose case has formed the subject of this history.

There must inevitably occur cases in which the state or condition of the individual will be intermediate between the milder and graver forms, but

in which, nevertheless, the spinal irritation will be quite unmistakeable : in such cases there ought frequently to be good grounds for hoping that judicious management may prove at least of some service, and I should be more disposed to trust, in these instances, to the effects produced by "derivatives" for the accomplishment of the object in view, than to any other proceeding whatever.

Setons, issues, and the actual cautery I have tried, and found to be of no small service, whilst "ice-bags" have been, according to my experience, of but little if any use, and sometimes I have thought that they served to aggravate the symptoms.

In cases of the kind I am now alluding to, drugs seem incapable of producing any permanent good whatever.

HISTORY VII.

PELVIC HÆMATOCELE.

About sixteen years ago M. Nèlaton published some "Clinical Lessons" on the subject of "Pelvic Hæmatocele," and it is not a little singular that before then there was no author (not even those who practised midwifery and the diseases of women as their speciality) who had ever made any distinct and definite reference to this almost common affection (accident?). That the writers and their text books should have been so reticent upon the subject before the period mentioned, seems to be quite inexplicable, for the malady must have been met with then as now, and must, one would imagine, have been not less frequent than we, at this time, know it to be, and the symptoms sometimes accompanying it quite as grave in their character. Could such silence have resulted from the fact that the ailment was not recognised, and that the state of those suffering from it was attributed to some other and, of course, erroneous cause? If so—what was the treatment adopted,

and what were the results produced by it? We may, perhaps, be pardoned for feeling some little anxiety on this latter point, for it has usually been taught that the effect of treating a disorder as being of one particular kind, when in reality it is something of a very different nature, is to promote a termination not generally of a very satisfactory character.

It is now very usually asserted that the symptoms and character of the complaint are always, or at least nearly always, so pronounced and pathognomonic that an error of diagnosis should be a very rare occurrence, but nevertheless the whole pathological facts connected with the complaint are not, as yet, satisfactorily established.

Many of the cases that have of late years fallen under the notice of the various, and now numerous observers, amongst whom we may name M.M. Recamier, Velpeau, Bermutz, Trousseau, Gallard, and Doctors Genonville, Tilt, Puech, M'Clintock, West, Duncan, and others, have ended in recovery, and in such instances it could not be said with anything like an absolute certainty under what anatomical conditions and into what parts the hæmorrhage had taken place, but in those cases in which death has ensued within anything like a reasonable length of time from the appearance of the first symptoms, and a *post mortem* has been made, the position of the hæmatocele, as far as its anatomical relations are concerned, has been clearly

made out, and in the greater number of cases this has been found to be intra-peritoneal, but still the opportunities afforded, at present, for thoroughly inspecting the pathology of the parts concerned have not been sufficiently numerous to admit of any very definite opinion being arrived at, beyond the fact of the effused blood occupying either an intra or an extra-peritoneal position.

It must, also, be taken into consideration, that as hæmorrhage into the peritoneum would be more likely to be attended with fatal consequences than when taking place external to this membrane, so likewise would facilities for examination of the pathological condition of the parts be more numerous under the former than the latter circumstances, and, consequently, it would be wrong to pronounce absolutely which of the two is the more frequent position for the bleeding to take place in, until by a careful collection of well observed cases, not only of those who die, *but also of those who recover*, more light is thrown upon the subject than we are now in possession of.

Although the diagnosis of this complaint has been spoken of as being particularly easy, and that any mistake upon the subject is *now* but *little* likely to be made, yet something may depend upon the period of the case when first seen by the physician, and also upon the accuracy, or the reverse, of the previous history of the patient which he obtains. As illustrative of this I give the following instance.

CASE.

E— P—, a married woman, 52 years old, was admitted an in-patient at the Stafford Infirmary on August the 22nd, 1863. She was rather short in height, and tolerably stout in person. Her complexion was somewhat sallow, and cachectic, resembling indeed the general look of those females who have the misfortune to labour under chronic, or malignant, uterine disease. She described herself as having had good health all her life, except upon two occasions: on one of these she had fever, of what kind she did not know, and on the other erysipelas of the leg; but she was not on either occasion ill for any great length of time, and was not considered to be in any very great danger.

She said that she began to menstruate when she was *seven years old*, and kept two monthly periods. She stopped and re-commenced menstruating when *eleven years old*, and continued quite regular to a day until she was nineteen years old, when she became pregnant. She bore a large family, ten in number, her last child being born when she was in her forty-fifth year. Her catamenia had not ceased up to the time she was admitted at the Infirmary.

She was in the habit of assisting to support the family by going out to wash; and about a year prior to her admission she was seized, whilst standing at the wash-tub, with a violent fit of abdominal pain,

which lasted for three or four hours. It was the time when her menses should have appeared. She with some difficulty got home, and was so ill for several days that she could not leave her bed, and was attended by a medical man. Soon after going to bed she began to menstruate, and, to use her own words, she was "nearly flooded away." The flux at length ceased, and she was able to get up, but "felt very weak and bad." For the first time she now observed that there was a round substance in the abdomen near the right illiac region; it felt very tender, and this, too, was the case with the whole abdomen, so that she was unable, she said, to bear the pressure of her stays. The tumour was represented as having been nearly round and hard; it remained of about the same size until her next monthly period, when it appeared suddenly to get larger.

She suffered more than usual at this time, and her "loss" was nearly as great as on the previous occasion. From this date she every now and then was troubled with considerable floodings, occurring at other times than when menstruating—the tumour, too, becoming larger and larger. She had sought for medical advice in several different quarters and had been told "that she might be in the family way and about to miscarry, or that she had miscarried—that she was suffering from dropsy—that she had a tumour of the womb—and lastly, that she had cancer of this organ."

She kept getting weaker and the tumour larger up to the time she entered the Infirmary. When I saw her for the first time her pulse was small and feeble, and beat 98 in the minute; her tongue was foul, especially at the back part of it, and had a "sodden" appearance; her urine was scanty and high-coloured, it deposited lithates, and had a specific gravity of 1.025, there was no albumen to be detected in it; her bowels were obstinately constipated, and she passed her evacuations with difficulty and not without pain; she also complained of a continual feeling of "bearing down," and often had the desire to defæcate without the ability to accomplish it; her appetite was bad and her thirst considerable.

On examining the parietes of the abdomen I found them to be distended considerably, but not equally, the distension being more marked on the right than on the left side; on manipulation a large, hard, but elastic tumour could be felt, apparently arising from the right illiac region, extending above the umbilicus and to a considerable distance on the left side of the abdomen; it was rather, but not very, tender upon being pressed, and the patient represented that she suffered an indescribable feeling of pain in it coupled with a sense of great distension.

A digital examination, per vaginam, being made I found that the os uteri was quite natural, but at the side and to the right of it there was a fixed

and unyielding protrusion which seemed to press strongly backwards upon the recto-vaginal septum, the uterus being forced somewhat upwards and to the left side. At the time this examination was made I had not obtained all the information I subsequently got, as to the manner in which the tumour first made its appearance.

It is really astonishing the difficulty frequently encountered in endeavouring to get *all* the particulars one wishes for from patients of the poorer class, at least, such is the case in the provinces; this woman I had questioned, and cross-questioned, for the express purpose of learning *how* her ailment began, but all I could get from her was "that she never felt anything amiss with her until she found a swelling at the lower part of her body;" she was full of information about the length of time she had been ill, the suffering she had undergone, and the various opinions that had been expressed as to the nature and probable termination of her case, but I failed to get any other account of the way in which her illness *commenced* than the one just mentioned.

After the vaginal examination I was somewhat disposed to regard the case either as being one implicating the right ovary; or that, having regard to the repeated floodings, there might possibly be a fibroid tumour of the uterus. To assist my diagnosis as to this latter affection, I was purposing to introduce the uterine sound, when casually—

certainly not purposely—happening to ask if she had ever hurt herself by any accident such as a fall, I got the following reply—“No, sir, but one day when I was at the wash-tub wringing out clothes I felt something give way inside me, and I was in such dreadful pain I thought I should have fainted and fallen down, and I was in terrible suffering for a good many hours.” I need not say this gave a different colour to the whole case, and when I remonstrated with the poor creature for not having told me this before, she answered, “yes, sir, but you know it was more than a week after this before I felt any lump, and you only asked when my illness *with the lump first began*.” I had now obtained the important point in the case, and shaped my course accordingly.

As my patient seemed to possess a fair amount of stamina, and as a second digital examination convinced me that no puncture of the tumour could be made without incurring what, to my mind, would have been an unjustifiable amount of risk, I resolved to watch the case carefully and to treat the general symptoms upon general principles, and not to puncture unless as a matter of the extremest expediency.

She continued in much the same state as when admitted for about a fortnight; during this time it was found necessary to give her aperients tolerably often, for although the passage of an evacuation gave some pain it was always more severe when the

fæces were hard, and besides this, she was always more free from suffering, and altogether more comfortable, after her bowels had been well relieved: as an anodyne I found the tincture of Indian hemp to suit her better than anything else. Her appetite improved, and she was supplied with a liberal and easily digested diet.

At the expiration of a fortnight from the time of her admission was her monthly period, and the discharge set in most profusely—in fact it amounted to absolute flooding—it continued for three or four days and then gradually, but slowly, subsided. During the time of the discharge the tumour evidently got larger, but as evidently became somewhat less when menstruation ceased. The loss of so much blood had rendered her very weak and her appearance even more anæmic than it was at the time she entered the institution. She was allowed an extra quantity of stimulant, and fed upon anything that she asked for, if it were not likely to disagree with her.

In the course of a very few days she revived considerably, and looked very much as she did when I first saw her; she spent a good deal of her time in bed, but got occasionally into the day-ward, and sometimes even into the garden, but she now seemed to suffer more from the “bearing down” sensation which has been before mentioned: her condition, however, was pretty nearly the same as when she entered the Infirmary, excepting

that she seemed stronger and her general health improved.

This state of things continued until the return of her catamenial period, when the same excessive flow as before commenced, and it became imperative that she should not leave her bed; the tumour once more became larger and the abdominal distension was much increased. By degrees she ceased to lose so much blood, and about the sixth day bleeding had altogether stopped; although very weak she did not appear so exhausted as at the last menstrual period; the tumour did not, as on the last and previous occasions, lessen in size, whilst the feeling of distension and bearing down was worse than it had ever been.

I now made another vaginal examination, but beyond an *increased* feeling of hardness and general immobility of the tissues at the upper part of the vagina and to the right of the os uteri, there was no difference from the state of the parts disclosed by my first examination.

On visiting my patient the following day the nurse acquainted me that E— P— had commenced being unwell again, and that the colour and appearance of the discharge were different to what we had before seen—the quantity was not described as being considerable, but that a continual draining was going on. A direct examination showed that the discharge consisted of a thick dark-coloured fluid, almost of the consistency of treacle, the odour

being most peculiar, but, at this period, not offensive; the patient expressed herself as feeling certainly no worse but if anything better, the sensation of distension being less, and the "bearing down" diminished.

This discharge rather increased in quantity during the next day or two, and became more blood-like in character, but at the same time infinitely more disagreeable in odour; very shortly, that is to say at the time I paid my next visit, I found that the discharge had again altered, it was now sanguino-purulent and terribly offensive. Several days elapsed before this kind of discharge altered for the better, but at length it did do so and became almost watery and occasionally streaked with blood, and the smell proceeding from it much more tolerable.

During this time the patient's general condition became much improved, her appetite got better, her tongue was cleaner, her bowels more regular, and she quite lost the annoying and painful sensations of "distension" and "bearing down."

There was no particular alteration made in her treatment beyond the withdrawal of the sedatives, which she no longer needed; the vagina was repeatedly syringed out, sometimes with a mixture of carbolic acid and water, and sometimes with M'Dougall's carbolate of lime and water, both accomplishing satisfactorily the object they were intended to effect. The patient still looked very

cachectic, but, altogether, incomparably better than she had done at any time since I first saw her. By the 12th of October she had only a very trifling discharge, the tumour, and of course the abdomen, were very much reduced in size, and as she expressed a strong desire to go home she was on this day discharged "greatly relieved."

I subsequently had numerous opportunities of seeing her, and on each occasion found a most marked and satisfactory improvement in all her symptoms until at length she declared herself to be "quite well." The tumour had disappeared, and on my last interview with her, the only thing detectable was a kind of diffused hardness of the tissues in the right illiac region: this could only be felt through the abdominal walls.

COMMENTS.

This case was not one which upon a first glance, nor indeed after a carefully made examination, admitted of being diagnosed with anything like extreme confidence. The length of time that had elapsed since its commencement, together with the imperfect history at first obtained from the patient, tended considerably to obscure the nature of the disease.

In spite of the frequently repeated assertion that there should be but little if any difficulty in arriving at a correct diagnosis of the complaint,

yet my experience teaches me that such a dogma requires considerable modification—true it is that the majority of such cases are readily enough made out, but there is a wide difference between those cases which from their sudden appearance, and the acute symptoms attending them, leave but little room for doubt as to their nature, and those which occur in a more slow and insidious manner and whose course takes on a phase that may truly be called “chronic;” but still there is yet another class of cases, which although occurring suddenly and having symptoms of an acute and alarming description, yet run a chronic course, just, in fact, as was the case with my patient E— P—. In either of these *chronic* forms there may be circumstances which render a diagnosis not altogether such a very easy matter as some writers upon the subject would endeavour to make it out to be.

The whole of this subject—*i.e.* “hæmatocele” —was exceedingly well handled by Dr. M’Clintock in an article of his which appeared in the *Dublin Quarterly Journal* for May, 1863. In his very able work on “Diseases of Women,” published in the same year, he selects the term “pelvic hæmatocele” in preference to “uterine hæmatocele,” as he thinks the former title is best confined to extravasations of blood within the pelvic cavity, whether they occur inside or outside the peritoneum, whilst the latter name is more properly confined to extravasation of blood *into the substance of the*

uterus itself. But in whichever or whatever position the hæmorrhage takes place, it must be recollected that the hæmorrhage, after all, is only a *symptom*, and that some pathological condition of the parts must have existed to allow of such a symptom occurring.

The sources of the hæmorrhage are necessarily no more uniform, or universal, than is the part or position in which it takes place—sometimes it may proceed from a hyperæmic condition of the ovary, sometimes from simple exhalation from the mucous membrane of the fallopian tube; or, it may come through the duct direct from the uterus. A rupture of the utero-ovarian vascular plexus might occasion it, or it might be produced by a simple, but general sanguineous exhalation from the peritoneal surface: there are doubtlessly other sources from which, and other positions into which, hæmorrhagic outpourings might take place so as to constitute a “pelvic hæmatocele,” but enough have been named to show how numerous may be the sites from which the blood may issue, and how different may be the actual position of the hæmatocele.

It is customary with some authors when writing upon this affection to designate the “hæmatocele” as being “retro-uterine,” “peri-uterine,” or “uterine,” just according to its actual, or imagined, location. To me, however, it seems that Dr. M’Clintock’s division into pelvic and uterine hæma-

tocele is both simple and sufficient, whilst the fact, if it can be ascertained, of the hæmorrhagic accumulation being extra or intra-peritoneal is the practical point to be kept in view as regards the treatment and prognosis.

In almost every recorded case of this disorder the cause of it has been able to be traced to some irregularity, or some interference with, the menstrual function, and although it can be easily believed that other causes might operate in the production of such an ailment, yet the one referred to is undoubtedly both the most likely and the most frequent occasion of it.

The symptoms which accompany the formation of hæmatocele will naturally vary according to the various localities from which the hæmorrhage takes place and into which it flows—they may be of the most sudden and alarming character, intense abdominal pain, rigor, an almost absent pulse, pallor of countenance, cold extremities, in short, all the appearances of approaching dissolution, indeed in some cases death may occur with terrible rapidity.

Between these immediate and alarming symptoms and those slow, insidious, and milder sufferings I have previously alluded to, there may be all manner of degrees, the differences consisting *broadly* in being acute, or chronic, or in having been acute and become chronic. To this last kind I consider my case of E— P— belongs, as her

description can lead to no other inference than that the attack she experienced when standing at the wash-tub was the commencement of the acute stage of the malady, which subsequently lapsed into the chronic form, and in which condition she first came under my notice.

As will have been observed by what I have already said as to my diagnosis of this case, I was guided in coming to the conclusion which I arrived at, principally by the woman's description of the manner in which she was first attacked: had it not been for this portion of the history of the case there was nothing to guide me as to the nature of the malady, and certainly no physical examination afforded evidence sufficiently clear and positive to enable me to name the affection with anything like an approach to certainty.

Having determined in my own mind the nature of the complaint my patient suffered from, there still remained to be solved the question as to whether the "hæmatocele" was extra or intra-peritoneal, and this I think is, in many instances, the most important and difficult point to decide. The same obtains with regard to pelvic cellulitis, pelvic abscess, and moreover this state (I mean pelvic abscess) is one, truly the most likely one, for hæmatocele to be confounded with; and perhaps formerly, before the communications of M. Nélaton, all pelvic hæmatoceles not absolutely *uterine* were mistaken for pelvic cellulitis and its

effects. The anatomical conditions of both these complaints are a long way from being satisfactorily established.

As regards "cellulitis," M. Nonat is the great defender of its extra-peritoneal position and M. Bernutz the supporter of its intra-peritoneal seat, but as to the extra or intra-peritoneal hæmorrhage in hæmatocele we have not up to this time any sufficiently numerous and well observed number of cases recorded to guide us in the matter; and besides this, there must, under all circumstances, be many and great difficulties in the way of our diagnosing with precision the exact position of these collections of blood, and to be able to say with any degree of certainty during life that they are or are not enclosed by peritoneal membrane.

There is great diversity of opinion as to the propriety or not of puncturing in a case of hæmatocele. Some are disposed to puncture *if* the tumour be extra-peritoneal, adopting a different or rather an opposite course if it be intra-peritoneal; but the cases are many in which you cannot possibly tell which of the two positions the tumour occupies. Others are not inclined to resort to the trochar unless *compelled* to do so by the urgency of the symptoms—this I own is my feeling on the subject, and upon it I acted in E—P—'s case. With the result I had no reason to be dissatisfied.

HISTORY VIII.

EPILEPSY FROM PERIPHERAL IRRITATION.

In No. 12 of Guy's Hospital Reports, published in 1841, there is an article on Epilepsy by Dr. B. G. Babington, in which, at the commencement, he expresses his object to be "to give some illustrations of the disease (? *disorder*) itself, and to offer some reasons for thinking that it depends on a functional, not on a structural change, and some grounds for the belief that in many instances it admits of cure."

Now, looking at these "reasons" as embodying the expression of an opinion, it is satisfactory to think that it receives the endorsement of, I believe I may say, the majority of the profession—for the result of experience has gone far to prove that although cases of epilepsy and its sad effects do occur in which any and every effort for their cure prove only our utter inability to be of any real service, yet that there are instances where, the *cause* of the functional disorder being discovered, remedies do act, and that too in such

a satisfactory manner as altogether to restore patients to health, and entirely free them from this hideous malady, admits of no doubt whatever—of course cases depending, *as some undoubtedly do*, upon organic alteration of structure cannot be expected to yield to such means as are at present known, and in cases in which the *cause* of the functional disorder—being only *functional* disorder—remains undiscovered we must admit, with humiliating feelings, that our blows at the affection are given at random and in the dark, whilst any success that may ensue from the treatment, must be honestly admitted as being the effect of chance rather than of skill.

It may not be amiss just to run over some of the causes which are almost universally admitted as being likely, or at any rate able, to induce epileptic seizures :—Scrofula, debility, excessive discharges, mal-formation of the head, intemperance, passion, fright, suppressed discharges, excessive emotional sensations, excessive sexual intercourse, masturbation, plethora of the cerebral vessels, the very opposite condition of the same vessels, nervous irritation of almost all kinds, both centric and eccentric, tumours in the brain, parasites in the same position, and lastly, the ingestion of certain narco-irritant poisons.

These are a goodly number, but there are yet others which might be mentioned to swell the list to a much greater length; enough, however, have

been named to afford the opportunity of reflecting upon the many opposite states the existence of which are capable of originating the same affection, and beyond this, it may not be altogether useless to have our attention directed to the many causes of the disorder which may rightly be considered as avoidable, and also to such as appear to be of an unavoidable nature; the former are by no means few, indeed they probably form the majority, whilst the latter are so generally dependent either upon hereditary pre-disposition, or what, I think, may be termed the "*force of circumstances*," that their entire prevention is beyond all ordinary or extraordinary control.

It is by no means a reason for congratulation that whilst the avoidable causes are so numerous, yet, from the very weakness of human nature, our means of prevention are so limited; and thus it happens that our Lunatic Asylums afford at one and the same time a sickening record of the vices of men and of the victims of epilepsy.

The following history is that of a case of epilepsy produced by peripheral irritation, and there are circumstances connected with its *cause* which would have altogether prevented my relating it had my book been at all likely to be perused by any other than members of the profession; but as it exhibits in a strong light the success of treatment in a severe case of epileptic convulsions, the success being dependent on the

discovery of the cause, I am tempted to think that an account of it may be not altogether without some usefulness.

CASE.

I— H—, age 18, a blacksmith by trade, was admitted as an in-patient at the Stafford General Infirmary, on Saturday evening, October the 22nd, 1864; he was brought in a cart, not being able to walk in consequence of the frequency of his fits.

A rule of the Infirmary forbids the admission of epileptics, but as the patient lived some miles distant, and had a fit as soon as he was brought into the waiting-room, the House-Surgeon sent for me to see the case (it was my week), and to know what had best be done with the man.

I went as soon as possible and found the patient on the floor of the room, convulsed, frothing at the mouth, the face livid and frightful, the eyes rolling, and the mouth somewhat on one side—the characteristic convulsive movements occurring again and again.

He was perfectly unconscious, and altogether presented a condition exactly resembling that given by Dr. Watson, in his lectures on the Principles and Practice of Physic, when describing the diagnosis of epilepsy and hysteria.

Some time afterwards, on recovering from the fit, he knew nothing whatever of anything that had transpired.

He was a well made young man, with all that muscular development of chest and arms usually found in those who follow his trade. He informed us that he had had fits at times for the past two years, but that during the last fortnight he had several in the course of each day—not more than three or four hours intervening between the fits. This alarmed his friends so much that they brought him to the Infirmary.

His pulse when out of the fit was weak and frequent; his bowels he described as being regular, and his appetite tolerably good; his habits were spoken of as being temperate. He complained of intense pain in the left side coming on occasionally and preceded by a sensation, he said, of “a lot of marbles rolling up and down” the descending colon—it is to this pain *he* ascribes his fits.

He looked pale, and in his manner and replies was particularly shy—very unlike what one would have expected from a blacksmith—his eyes appeared “watery,” and altogether there was that about him which induced me to put questions obtaining from him an indirect admission that he practised a vicious and disgusting habit to a most pernicious extent.

I should have mentioned that when examining his side he flinched and said that the pain was so great he could not tolerate any pressure, but his attention being called off he allowed me to handle his side roughly enough, so that this symptom one

was disposed to put down to "hysteria" (that is to say if, looking at the derivation of the word, one may apply such a name in connection with the male sex) and not to anything like an "aura epileptica."

Believing myself "master of the position" I ordered him to be placed in bed, and although I did not think it altogether prudent to direct the amputation of an organ the analogue of that which it has been proposed to "snip off" (and which has actually been removed) for the cure of epilepsy in the female, yet I directed the part to be well blistered and left him for the night.

On the 23rd—Sunday—I saw him and he had had no fit; his diet was milk and beef tea; he had no medicine.

I saw him again on the Monday—still no fit, he complained only of weakness and the soreness of the blister; ordered full diet, and phosphate of zinc with dilute phosphoric acid and compound tincture of bark.

Day after day he improved in point of colour and general appearance; he lost his shyness, and left the Infirmary three weeks from the time of his admission without having had a single attack from the time the blister was applied.

With the exception of a dose of aperient medicine, his entire treatment consisted in what I have described; the soreness was maintained during the whole time he was in the house, and I directed a fresh application to be made on the day he left.

Some months afterwards I had the opportunity of learning that he had had no return of the fits, and since then I have been informed that he has married and has continued quite free from his epilepsy—a most fortunate termination—but how very rarely do we find epilepsy of two years' standing so suddenly, so rapidly, and so permanently removed!

COMMENTS.

The several causes that may operate in the production of an epileptic condition, and in many, indeed in most cases, the obscurity that surrounds them, has undoubtedly occasioned the numerous theories that have been promulgated as to where, how, and what the evil is that conduces to such a dire misfortune, and from the same reason one infers must arise the great difference of opinion which exists as to the best means to be used for the cure of the disorder—how else can we reconcile such opposites and extremes of practice as sincipital combustion recommended by M. Gondret, and ice-bags to the spine, directed by Mr. Chapman?

It is only justice, however, to the former gentleman to bear in mind that in his treatise on "Derivation," published in 1837, he adopted the well-known aphorism of Hippocrates, "Diseases which medicine does not cure steel cures, those

which steel does not cure fire cures, and those which fire does not cure must be regarded as incurable."

Then again, only look at the various medicines possessing opposite qualities that have from time to time been recommended as "cures" for this horrid affliction—tonics and purgatives, stimulants and depressants, excitants and sedatives, have each in their turn had their admirers, so that one is inevitably driven to the conclusion that such diversity of treatment has been occasioned by the fact before stated, viz.: the many different causes which may occasion the complaint and their hidden and mysterious nature.

Pinel in his work on Cerebral Pathology perhaps gives as true, concise, and graphic an account (although by no means novel) of the malady as can be found in any author—he says, when speaking of the nature and seat of the disorder, "As in the case of general paralysis so of epilepsy, the identity of seat so vainly sought for can only be explained by supposing the morbid state to implicate certain fasciculi, or fibres of certain of the more irritable portion of the encephalon. Hence that morbid state may be seated in the medulla oblongata, in the pons or cerebellum, or in the brain itself.

"It may depend upon a state of chronic inflammation, or simple chronic irritation, or atrophy or hypertrophy, or effusion or the presence of a

tubercle, and, in a word, upon all lesions, trivial or serious, which are seated in the site of these motor and sensitive fasciculi or fibres."

This, written by Pinel in 1844, is quite compatible with anything that has appeared from the pen of a Brown-Sequard, a Radcliffe, or a Ramskill of late years.

True that the micrology of the nervous system was not so perfect as at present, but for all practical purposes the ideas of Pinel in 1844 were as correct as to cause and effect, and as serviceable in directing the treatment of epilepsy, as anything that has emanated from the learned and laborious treatises written by the three distinguished authorities I last referred to.

Dr. Radcliffe strives hard to prove that *all* convulsion is the sign of depressed and not of exalted vital action; and in proof that it is not caused by fullness of the cerebral veins he very properly refers to the experiments of Kussmaul and Tenner, but although *as a rule* he may be right in this opinion, and I really think he is, yet with so many causes in operation to occasion the disorder there must be *exceptions*, and I cannot consider it judicious to generalize as to the essentiality of one condition in occasioning symptoms acknowledged (*vide* Pinel) to arise from such different and diametrically opposite states.

In looking at the symptoms and treating the case I have given a history of, I could not (when

my attention was drawn to the pain in the side and the feeling in the colon described by the patient as preceding the attack) help remembering the description given by Ramskill of what he calls ganglionic epilepsy, and in which he mentions a disturbance of sensation accompanied by an unusual feeling of motion in the abdomen occurring as an "aura," and which he considers to be the result of a disturbed condition of the solar plexus, and the ganglionic system of the abdomen generally.

I was half disposed to attribute the condition of my patient to this ganglionic state, but his oversensitiveness to being touched, and his subsequently allowing me to make firm pressure without complaining, pointed, I thought, to another cause, and I had no reason to regret having left the "aura," if it were one, alone.

HISTORY IX.

EPILEPSY FROM HEPATIC CONGESTION.

Cases of Epilepsy are generally, at least in provincial Hospitals, "*tabooed*," the rules strictly prohibiting their admission. This, I think, arises more from the circumstance that a tolerably universal belief pervades the public mind that the disorder is incurable, than from any disposition to avoid the trouble and annoyance which such cases not unfrequently entail.

Of course cases of very long standing, whether resulting in the first instance from organic alteration of ultimate structure, or from having lapsed into this condition in consequence of a protracted continuance, are very properly excluded from admission at any institution except such an one as may be especially intended for epileptics; but recent and first attacks of the complaint ought, I consider, to have a chance afforded them of being benefited by treatment in any General Hospital, and to admit of this being done a latitude should be given to the medical officers as to the taking in or rejecting of such patients.

Some of our highest authorities on the physiology, pathology, and treatment of epilepsy—I need only name Van Der Kolk and Brown-Sequard—assert that many, if not most of the recent cases of the disorder admit of being cured; and the former authority in his work “On the Spinal Cord and Medulla Oblongata” (translated by Dr. Moore) at page 255, makes the following observation: “Far as I am from being able to undertake to cure every subject of epilepsy—as unhappily the majority, after having in vain employed all kinds of remedies, at last seek assistance when the period of recovery is already past—I am convinced from five-and-thirty years’ experience in the examination and treatment of very many epileptic patients, that in the commencement, as I have just now stated, epilepsy is a disease which, at least in the great majority of cases, is very capable of rational treatment.” Subsequently he gives cases illustrative of the success which resulted from this “rational treatment,” satisfactorily showing how stable was the ground on which he had based the conclusion I have quoted above.

Dr. Sieveking, in his admirable “Croonian Lectures” on the “Localization of Disease,” delivered before the College of Physicians, says, “that all local disease is an expression of malnutrition, and that our remedial measures act curatively only by influencing the nutrition of the body:” this is tantamount, I imagine, to an acknowledgment of

the "unity of disease," and that our remedies, whether preventive or curative, must, or should be, directed especially to the purposes of nutrition. Such an opinion is to my mind deserving of the greatest respect and attention, as I am convinced that it will be found to be correct in principle and serviceable in practice.

Dr. Sieveking pertinently enquires, "What is the antecedent of hepatic disease but a derangement of nutrition?" and the same enquiry might be made as to pneumonia, and, in fact, as to any of the disorders with which we are acquainted excepting those that may be of parasitical origin. This view is one that the profession seem well inclined to adopt almost unanimously; and hence has arisen of late years that great attention to the sanitary condition both of the healthy and sick, and a perception that whatever drugs may be administered the result of treatment must depend almost entirely on the care bestowed to purity of the atmosphere, personal cleanliness, ventilation, selection of food, adaptation of exercise, in short to all those surroundings and management most likely to effect and promote in a favourable manner the general purposes of a "*healthy nutrition*."

This has been, most improperly I think, styled the "Expectant Method;" but, as I have had occasion elsewhere to remark, disorders and diseases are treated *medically*, not by drugs *alone, or even at all*, but by all the means at our command for the

accomplishment of a rational curative effect; and as we live, so to speak, by nutrition, as all our ailments arise from errors of nutrition, so attention to nutrition would seem to be the proper and principal indication in the treatment of every malady; and, in this light, *food* becomes *physic* when judiciously selected, and it may, in the same sense, act as *poison* when injudiciously and improperly chosen.

As the means by which the elements of nutrition are carried to each and every part of the human mechanism, the blood stands pre-eminently important; and that any interference with its normal constituents, may produce effects of the most sudden and serious character, we have almost daily evidence to convince us. Epilepsy is one of such effects, and the history of the case I am about to give will serve the double purpose of shewing this, and also the curability of the disorder.

CASE.

J— E—, a cab-driver, aged 22, was admitted at the Infirmary on January the 13th, 1864. He was short in stature, and of a light but well-made frame; his complexion was dark and his eyes large and very prominent; his habits had been dissipated, and for some months previous to his admission he had been in the habit of drinking spirits freely, but he said, he had rarely got drunk. He had had gonorrhœa, but never had syphilis.

He described himself as not having been well for several months, and that for the last two or three weeks he had suffered a good deal of pain in his right side just under the ribs; his tongue was very foul, particularly at the back part of it, his breath offensive, his pulse beat eighty-two in the minute, and was hard and thin; his urine was extremely high-coloured, scanty in quantity but clear, and the specific gravity was 1.025, its chemical reaction was slightly acid, and when treated with heat and nitric acid it afforded a slight evidence of albumen. The nitric acid and Pettinkoffer's tests showed bile to be present.

Examined microscopically, no urinary tube-casts could be seen, but there were a few epithelial cells coloured with bile pigment.

His bowels were constipated and the evacuations very pale in colour. The skin was hot and dry, and he complained of being thirsty; the respiration was twenty-two in the minute; his complexion had a particularly muddy-yellowish look, and the conjunctival membrane was suffused with bile.

An examination of his chest afforded no evidence of the lungs or heart being at all affected; the former acted freely and well, the respiratory sounds being perfectly natural, and the first and second sounds of the latter were quite clear and distinct, with a properly maintained rhythm; both sides of the chest expanded alike.

Pressure made on the lower ribs on the right side, and also over the epigastrium, gave considerable pain, and the vertical dullness on percussion over the region of the liver exceeded five inches; deep inspiration occasioned a darting, lancinating kind of pain, which extended from the liver back towards the kidneys.

I considered that my patient was suffering from a hyperæmic or congested condition of the liver, and ordered him to have a brisk dose of podophyllin; also a mixture containing the phosphate and carbonate of soda with compound infusion of gentian; eight leeches to be applied over the hepatic region and subsequently a blister on the same place; his diet to consist of farinaceous food and milk.

On my next visit, which I made the following day, I was told that I had scarcely left the Infirmary before he had a severe "rigor," followed by an epileptic fit; he had bitten his tongue severely and it was so sore that he talked with difficulty, but I managed to get from him that this was the seventh fit that he had had within a fortnight.

At the time of his admission he made no reference whatever to his having had fits of any kind; in fact he had, as he admitted, carefully avoided making any allusion whatever to them, fearing that if he did do so he would be refused admission: however as he was really very ill and already under treatment, I resolved to let him remain, at least for a time.

I was prompted to follow this course from the circumstance that the fits were of very recent occurrence, (he solemnly assured me that he had not had a fit at any time prior to the period mentioned,) and that I thought they might very probably be altogether dependent on the condition of his liver. The termination of the case proved that such was really the fact.

I ordered the diet and medicine to be continued in every way the same, except as far as regarded the podophyllin; this I directed to be taken in smaller doses every other night. The first dose had procured three or four copious dejections, almost white in colour, tolerably firm in consistency, and most offensive in odour. The urine was just the same as when previously examined, but his pulse was both softer and fuller, and beat seventy-eight in the minute. He had slept for some considerable time after the fit and still seemed very "drowsy."

For the next few days his state remained very nearly unaltered; he appeared to shake off the effects of the fit with considerable difficulty, and continued sleepy and stupid; his evacuations were beginning to improve in colour. The urine he made was lighter in colour and more in quantity but still contained albumen and bile.

The soreness of the blistered surface prevented, or rather interfered with, an examination as to whether the area of dullness over the site of the

liver had diminished or not, and for the same reason I could not tell whether he could bear pressure without experiencing a less amount of pain than he had done; he ate his milk diet readily enough, but said he could not taste it as he used to do; everything was "bitter."

On the eighth day from the date on which he had the fit I have been mentioning, he had another but a much milder one; it occurred whilst he was putting on some clothes to go to the water closet; he fell to the ground but did not hurt himself, and the attack was over in the course of a few minutes; he was quite unconscious at the time and screamed at the onset of the fit; his tongue was only slightly bitten upon this occasion.

He slept for more than an hour after the fit terminated, and on waking seemed immediately to be as he was before the fit.

His general condition had become much improved by this time; his tongue was much cleaner; his breath less offensive; his pulse was seventy-four in the minute, tolerably full and soft; his urine had a specific gravity of 1.020; it was acid, paler in colour, and quite sufficient in quantity; neither albumen nor bile could be detected in it; his bowels acted regularly and the colour was nearly natural; his complexion had almost lost the "muddy-yellowish look" that it had when he came into the institution, and the conjunctivæ were not nearly so yellow as they had been; the fullness and

tenderness over the locality of the liver were still there but considerably diminished, the vertical dullness on percussion had lessened to the extent of nearly an inch, it being now very little over four inches; a deep inspiration caused him no uncomfortable sensation of any kind beyond a little feeling of constriction just on a line with the ensiform cartilage; he took his food with some relish and described his "taste" to be returning.

I made no difference whatever in his treatment, the diet and medicine being continued. He expressed a wish to be allowed to sit up, and this he was permitted to do.

He kept getting better slowly but steadily until just eight days from his last attack, when he was seized with another fit whilst having his dinner in the day-ward; it was much slighter than his former one; the tongue was *not* bitten this time, neither did he scream when the fit commenced, but he was quite unconscious, and would have fallen from his seat had he not been caught and supported. He was taken to bed and he slept for a short time; when he awoke he went into the day-ward again, and had his tea there.

From the fits taking place just eight days from each other, I thought it likely that it would prove to be a case of periodical epilepsy, but the subsequent course of the case showed that such was not to be the character of the complaint.

His symptoms, one after another, became im-

proved, and although even now he could not tolerate any considerable pressure over his liver, yet the extent of dullness on percussion was so far diminished as to be but little beyond the extent that is natural.

The excretions were quite what they ought to have been, and his appetite was so good that he asked to have his diet altered, and to a certain degree I assented to this request by adding to his milk diet a pint of beef-tea daily. His complexion was almost healthy, but the conjunctivæ were, even at this time, slightly tinged with bile. The mixture that he had been taking I ordered to be continued, but the podophyllin I directed to be given only occasionally and in still smaller doses.

He continued to progress in the most satisfactory manner, although on the eighth day from his last fit he complained whilst walking in the garden of having felt "dizzy:" this feeling took place, he said, two or three times during that day, but he had no marked epileptic seizure and was never for a moment unconscious.

He never had another symptom of a fit during the remainder of his stay in the Infirmary. His appearance became much altered; he gained flesh and altogether had a healthy look: his complexion being clear, and the "whites of the eyes" free from the least tinge of bile. He now bore almost any amount of pressure over the liver, and there was no more dullness on percussion over this region

than there ought to have been, his excretions and secretions seemed quite natural, and his appetite was very good.

He was discharged "cured" on March the 12th, 1864, having been in the Infirmary eight weeks and two days.

Prior to his leaving I spoke seriously to him about his habits and mode of life. He expressed a wish to leave off "cab-driving," and to get into some respectable service as "groom." This I subsequently learned he succeeded in doing, and I had an opportunity of seeing and conversing with him about four months after he left the Infirmary: he was looking extremely well, and up to that period had neither had another epileptic fit nor anything at all resembling one.

In January of this year, 1866, I heard of him, and was told that he "had married, was very well, and had had no more fits," so that the permanence of the cure can scarcely be doubted.

COMMENTS.

Perhaps one of the most perfect descriptions, if, indeed, it be not *the* most accurate description, of the phenomena of epilepsy, not only as regards the milder or severer forms of it, but also of the phenomena which are more or less constant, may be found in the work of M. Herpin, "*Du pronostic et du traitement curatif de l'Epilepsie, Paris, 1852.*"

In his account of the various symptoms displayed in the accession, progression, and termination of an epileptic seizure, he carefully and elaborately brings before the reader all the various points that may shew either dependence upon, or an association with, the reflex action exercised, especially upon the larynx, pharynx, and the muscles of the face; and he then indicates the probability, if not the absolute certainty, of the whole being the result of the condition of the medulla oblongata. Subsequent authorities, Marshall Hall, Van der Kolk, Brown-Sequard and others, all point their conclusions in the same direction, and although in some matters that strike me as being mere circumstances of detail they seem to differ, it is rather in words than in facts that they disagree; whilst *all* seem willing to subscribe to the opinion that the initiation of epilepsy and convulsions may be traced to the medulla oblongata.

Even Kussmaul and Tenner in their learned Treatise on Convulsions, assign the starting point of epilepsy to this particular part of the nervous system; but, nevertheless, differing from Van der Kolk and the rest as to the manner in which, or rather the cause by which the effects are produced; Van der Kolk and those who support him maintaining that the medulla is injuriously affected by either an increased afflux of blood into the very numerous nutrient blood-vessels with which it is so freely supplied, or by a stasis of blood in this

capillary net-work; whilst Kussmaul and Tenner contend that it arises from an anæmic condition of the vascular system in this locality.

The two conditions are obviously opposites, but the results may be, and in point of reality *are*, the same; because, after all, under both circumstances it is the “nutrition” of the part that is disturbed—too much or too little blood being quite sufficient to unsettle the due functional performance of this very delicate and intricate portion of our nerve structure.

But still further—there is yet another form of “malnutrition” which may give origin to precisely similar symptoms—for instance “Blood Disease.”

Blood may be diseased from containing matter foreign to its normal constitution, or some of its ordinary elements may be present in excess, or on the contrary, they may be deficient. The latter condition is well seen in the epileptic attacks which arise from “Chlorosis,” in which the iron, or hæmatosin seems wanting. The former state may be typified by the presence of bile in the circulating current.

I consider that it may fairly be inferred that this was the case with J— E—, the history of whose illness I have just given; the bile being present in his blood as a consequence of considerable and serious hepatic obstruction, this again being produced by an error of nutrition, such error having been inaugurated by habits of intemperance.

HISTORY X.

ALTERNATING LEUCOCYTHEMIA.

Although the term "Blood Disorder" or "Blood Disease" is generally used to indicate a restricted class of maladies, a closer, a more rigid and searching investigation of the blood than has hitherto been accomplished would in my opinion lead to the conviction that in *every* departure from the standard of health (ideal though such standard may be), either some error in the relative proportions of the normal constituent elements of blood, or the presence of some foreign and abnormal ingredient exists.

Whether such error results from defective primary or secondary assimilation, from the retention of effete and disintegrated material within the system, or proceeds from the introduction of morbid matter from without, in no way alters the fact that *it is the condition of the blood which constitutes the affection.*

However, it is customary to make use of the expression "Blood Disease" as applicable to all

contagious and infectious disorders—excluding of course the “Dermatozoa” and the “Dermatophyta”—and also to such ailments as can be proved by the means we are at present able to command, to have as one of their essential pathological features, an unnatural or altered condition of the blood, either relatively or otherwise. With even such a scope as this affords, the number of “Blood Disorders” must necessarily be very great, including as it will do fevers, cholera, gout, rheumatism, anæmia, chlorosis, leucocythemia, and a large category of other ailments.

In some of these—fever for instance—the particular body, whose introduction into the vascular system serves to produce the symptoms distinguishing the disorder, has evaded every effort made for its isolation, the method also by which it operates being a perfect, and so far, an insoluble mystery.

The profession therefore has had to theorise on a large scale, not only as to what the poisonous material may be, but likewise as to the manner in which it acts; and hence the hypothesis of “Zymosis.”

This hypothesis in its turn is now likely to receive a somewhat rude shock by having to stand a trial of comparison with the, at present, unworked out idea of “Catalysis,” which, by an advanced knowledge of Animal Chemistry and Physiology, is being put forward, and that too with very fair probability of affording an insight into this great

secret in a manner that absolutely demonstrates the long vexed question—*How* do animal poisons produce their effects?

There has been no period in the history of medicine when the condition of the blood has not been looked upon as of the utmost importance; but whilst modern Pathologists,—from the time when MM. Andral and Gavarret afforded the first connected series of researches, illustrating the morbid variations of the natural component parts of this fluid as to quantity and quality, until the present date,—have laboriously worked at the various problems offered by diseased blood, it has been the good fortune of investigators in this our own particular day, to contribute largely towards the general store of information that we possess upon the subject; and this, no doubt, is principally to be attributed to the improved physical means which they have been able to call into their service.

Whatever Chemistry may have done—and we all know how much we are indebted to it—in assisting to make clear the Pathology of the Blood, the microscope has scarcely done less; and there are some forms of blood disorder in which this instrument *alone* has been the means of determining the state of this fluid in a manner so clear that, as far as the physical condition of it is concerned, nothing more appears to be required. This remark may with great propriety be applied to the state of the blood which is found, in many instances, to be

associated with certain disordered and diseased conditions of the liver, spleen, and lymphatics. I refer to the "Leucocythemia" of Dr. J. H. Bennett.

It is by no means certain, indeed it is very doubtful, whether this "Leucocythemic" state occurs *only* when one or all of these organs are affected. Cases of hypertrophy of the liver or spleen, or of both liver and spleen, have been under treatment at all stages of the complaint, without any such condition of the blood having been observed; so that there still remain many discrepancies to be accounted for.

It should be borne in mind too, that M. Bouchut some few years since pointed out the proneness to an excessive formation of white corpuscles in the blood as *not being peculiar* to organic diseases of the liver, the spleen, or the lymphatics, but as liable to occur in various forms of cachexia and chronic diseases.

I am unable to find any authority who goes so far as to assert that "Leucocythemia" has been known to be present, with its most serious symptoms, as the consequence of simple, uncomplicated, functional disturbance; and yet the case which I am about to relate would appear to place the possibility of such an occurrence almost beyond all manner of doubt, with this additional singular circumstance, that the "Leucocythemic" condition was not constant but alternating.

CASE.

Mrs. F—, the wife of a beneficed clergyman residing in Yorkshire, about 58 years of age, and the mother of six children, gradually became an invalid in the Spring of 1864. She was of about the average height, and rather inclined to be stout in figure. Her general appearance was at this time indicative of health that might be called "robust;" her complexion was fair, and disposed to be florid.

Previously to the period referred to she had had but little illness, excepting at the time of her "confinements;" but even her confinements had not been more severe nor her recoveries more protracted than is very commonly the case.

Her ailment commenced with a continual feeling of languor, some shortness of breath, and an indisposition, if not an absolute inability to take the amount and kind of exercise that she had been in the habit of doing.

Her appetite was not impaired, and she slept very well, although she expressed herself as feeling but little refreshed by her sleep, and always awoke with a sense of weariness, almost as though she had not slept at all.

Her bowels acted regularly but scantily, and upon one or two occasions she had lost some

amount of blood during defæcation; but she said she had no *external* piles, and although at one time, many years previously, she had suffered a good deal from internal piles, they had never bled at any period.

Her ordinary medical attendant very correctly gave her "strengthening medicine," and ordered her to take a more generous diet than she had been in the habit of taking; but—to use her own expression—she "gained no ground;" in fact she got weaker, and her breathing worse. She had no cough, her appetite was still good, and she certainly did not lose any flesh, although she had become very pale.

In the Autumn of the year, as she suffered from no acute symptom of any sort, she thought she would try if a residence for a few months at the sea-side would benefit her, and she went with some friends, first to Scarborough, and then to Blackpool. She spent at these places two months or more, but experiencing no benefit from the proceeding, she returned home feeling worse, if anything, than when she left.

Her friends had now become alarmed. She therefore went to Huddersfield, and availed herself of the advantage of Dr. Turnbull's opinion. He submitted her to a careful examination, and came to the conclusion that she had no organic disease of any kind, but that she suffered only, and altogether, from general debility. He prescribed for

her a course of steel tonics, and ordered her to continue her liberal diet.

It is proper also to say that he examined her for external piles, and found evidence of her having had them, but said that they were then, and had been, to all appearance, for a long time, in a perfectly passive condition. She continued the course of treatment suggested by Dr. Turnbull for several weeks.

Her inability to take exercise—her shortness of breath, in spite of every care, increased gradually. Her appetite failed, and she became troubled with flatulency: still, however, she did not lose her plump condition, neither did she, when quiet, complain of uneasiness or any uncomfortable sensation of any kind, but her face was getting a very cachectic look.

Under such circumstances it was not surprising that her naturally good, almost high spirits, should begin to give way; and that she, as well as her friends, should become apprehensive as to the result of her illness. At the solicitations of some connections she now went to Leeds, to consult Mr. Teale, who like Dr. Turnbull was unable to find any existing organic disease to account for her really alarming state.

Mr. Teale said that he thought she was suffering from "gastric irritability," but he could not account for her anæmic condition; he enquired as to whether she had ever suffered from piles, and was

informed as to what had taken place when she consulted Dr. Turnbull. Mr. Teale did not examine her, but prescribed for her some sedative medicine, the active ingredient being hydrocyanic acid.

In course of further time the symptoms gradually increased in severity, until at length she could not get up stairs without experiencing a most distressing amount of breathlessness; and to take a walk even of a few hundred yards produced a very painful state of exhaustion.

In 1865, having come on a visit to some relations in Staffordshire, she became, whilst staying with them, so very much worse that I was requested to see her. I may mention that Mrs. F—intended to have consulted me had she not been—accidentally, as it were—in the neighbourhood at this time.

Having before given an account of my patient's appearance when her illness commenced, it only remains for me now to say, that on my first visit, which I made on May the 12th, 1865, I found her to be, as far as mere flesh was concerned, in a good condition; in point of fact she appeared almost too stout, but as anæmic as it was very well possible for a person to be.

Her face and hands were perfectly blanched, and the finger nails looked almost blue. Upon examining the mucous membrane of the mouth, I found it to be nearly colourless, and upon turning down the lids of the eyes the conjunctiva was seen to be in a similar condition. Her pulse was 65

and feeble: the tongue colourless but clean. She seemed cheerful, but said that the least exertion—even to move from one room to another—made her actually “pant for breath.”

She was, at our first interview, sitting up and in the drawing-room. After what she had said about any exertion distressing her, I thought it better to defer the examination of her chest, &c., until I had the opportunity of making it when she was in bed, and therefore postponed it until the following day.

In reply to my enquiries, she said that her bowels acted regularly, but the evacuations were small in quantity, and rather clay-coloured. She said that she urinated without difficulty, but that there was a great difference as to the quantity of urine that she made. Sometimes, or rather on some days, she voided a great deal, on others but very little: it was, however, always clear, and *never* high-coloured. Her appetite was tolerably good; her thirst moderate.

She spoke of being occasionally troubled with flatulency, and said she frequently felt a desire for food which, when presented to her, she had no relish for. To my enquiry as to whether she had ever *any* pain *anywhere*, she replied by a negative.

The following day when I saw her she had, according to my request, remained in bed, and this afforded me the opportunity of making a thorough and careful examination.

The chest was large and in every way well developed, and yielded upon percussion only the sounds proper to be heard in each region of it. The pulmonary sounds heard upon applying the stethoscope were perfectly natural, and the respiratory movements sixteen in the minute, both sides of the chest expanded alike.

Upon listening to the heart, both first and second sounds could be heard with complete distinctness, but accompanying the systole was a not-to-be-mistaken murmur—this was audible over the entire region of the heart, but was heard plainest at the base of the organ, and extended along the course of the aorta; it could be heard also posteriorly in the neighbourhood of the spine, but not so distinctly, or rather not so loudly, as in front: it was the same sort of murmur that I have heard in some chlorotic cases, and in many cases of extreme anæmia.

Upon examining the abdomen there was nothing of importance to notice: the stomach and small intestines appeared to be tolerably empty, and the same might be said of the ascending and transverse portion of the colon, but the descending part of it gave a very dull sound when percussed, although the bowels were represented to have acted pretty freely some short time before my visit: gradual and deep-made pressure upon any of the abdominal viscera produced no pain, except that when I pressed somewhat strongly over the region of the

right lobe of the liver she complained of a little tenderness, and this part of the organ extended below the ribs, rather beyond its ordinary limits, but still not in a way to call for special notice to be taken of it.

The little exertion consequent on this examination had occasioned the pulse to rise to 90 and the respiration to 24 in the minute. I had had a specimen of urine sent to me for examination, but I could find no evidence of albumen in it, nor indeed of any other abnormal ingredient; it was pale in colour, and the specific gravity was 1.020.

I had taken some microscopical glass with me, and I obtained a fair amount of blood by pricking the finger. The quantity placed on the glass was sufficient to enable me to get home before it became quite dry, and I immediately made my examination.

The field of view was crowded with red corpuscles, some dispersed and others arranged in "rouleaux;" between these last were to be seen many white corpuscles, both large and small, the latter being far the more numerous.

It is right that I should add to this that although I speak of the white corpuscles being many, and mean thereby that they were far more in number than the 1 to 50 or 1 to 100 red corpuscles described by various authorities as being the proper and natural proportions of these two bodies, yet they were by no means so numerous as

spoken of by Dr. Bennett in his cases of "Leucocythemia," nor as I have myself seen in the same disease.

Not being altogether satisfied with this examination, I determined to take a microscope—a small one of "Nachet's"—with me on my next visit, so that I might view the blood immediately when taken, and at the bed-side of my patient.

I prescribed for her some pills, consisting of very small doses of podophyllin, ipecacuanha, and extract of hyoscyamus, one to be taken every other night, and a mixture containing tincture of the sesquichloride of iron and infusion of quassia. I also directed that she should take a nutritious but easily digested diet—animal food at least twice a day, with a liberal quantity of either Burgundy or Claret—and suggested the desirability of her not moving about any more than she could do with perfect comfort and without distress.

In the course of three or four days I visited her again, and she expressed herself as feeling a trifle better: she said she felt a little more strength, and thought she got up stairs without experiencing quite so much difficulty of breathing; but her appearance and the state of her pulse in no way betokened any improvement.

I now renewed some enquiries I had previously made as to the blood she lost when at the water closet, and after much difficulty got an admission partly from herself, but principally from a daughter

who was with her, that she rarely had an evacuation without losing some blood, although, as they said, it was generally very small in quantity.

I next made my second examination of the blood, obtained in the same manner as before, using a number one eye-piece, and an object glass of 250 diameters in power.

The field of view was, just as on the previous occasion, crowded with red corpuscles dispersed in various directions at the commencement of my observation, but, after a while, many of them became arranged in "rouleaux." I looked in vain for some little time before I could see a white corpuscle, and when, at last, I did find some few of them, they were not at all more numerous than they ought to have been, they were all of the large kind, and I counted one white corpuscle to about 90 red.

The red corpuscles seemed very full, and their outline regular and well defined, but there were more "free nuclei" than I ever remember to have seen before, and they were very pale in colour.

After what I had observed at my previous microscopical examination, I was a good deal surprised at the result of this one, and resolved to investigate the condition of the blood again in the course of a few days; in the meantime I requested that the treatment might be persevered with.

I next saw my patient on the 20th of May, and was pleased to find that she considered herself im-

proving; she told me that she had walked into the garden, and that she certainly did not feel so much exhaustion as she had done; still she had the same palid anæmic look, her pulse weak and 68; her bowels had acted regularly, and the evacuations were neither so clay-coloured nor so small as they had been. I was informed that only on one occasion, since my last seeing her, had there been any blood passed, and then only a very small quantity.

I once more examined some blood microscopically, and it presented pretty nearly the same appearances as those observed on the second examination; this was so far satisfactory, but I was quite at a loss to account for the presence of so many white corpuscles in the first specimen that I examined, and the very numerous free nuclei in the second.

I again had a small quantity of urine to examine, it was in point of colour and odour perfectly natural; it yielded no evidence of albumen with heat and nitric acid, neither was there any sediment; the specific gravity was low, 1.013.

I did not see Mrs. F— again until the 25th, she was far from feeling so well as when I saw her on the 20th, her weakness was evidently greater, she had not been able to go out again, at least not to walk, but she had been out in a carriage, she was both looking and feeling very languid, whilst her general expression was, if possible, even more cachetic than it had hitherto been; her pulse was

extremely small and beat 61 in the minute, she was in bed and in the recumbent position.

I was given to understand that she had lost some blood whilst at stool during the last three or four days, and, when out of the bed-room, her daughter acquainted me with the fact that upon one occasion the quantity lost was rather considerable. She still had no acute symptom—no pain at any time or of any kind.

At this time I ventured to express an opinion that her whole condition was most probably dependent on the presence of internal piles, or some more unfavourable condition within the “rectum;” and I pointed out the necessity of her submitting to a proper examination; at the same time saying, in reply to enquiries, that it was not at all improbable that an operation of some sort would have to be performed, as it would not do to have such repeated losses of blood, feeling, as I did, pretty sure that she had been losing a much larger quantity of this fluid than she was, or had been, at all aware of.

She replied that as she had made up her mind to go to London, she would like me to give her a letter of introduction to some physician in town who had made the blood an especial object of study, as she would like to have his opinion before seeing a surgeon; and as I knew no one who had devoted himself more to this subject than Dr. B. W. Richardson, I complied with her request, by giving her a note to him.

In my communication to Dr. Richardson, I drew his attention to the various circumstances and points of the case, which in the course of my description I have already made mention of, and asked him to oblige me with a few lines as soon after his interview with my patient as he could make it convenient to write to me.

Not very long after this I received a letter from him, which I here give at length. He said, "I have seen Mrs. F— now four times, and have studied every point to which you so carefully directed my attention.

"Beginning by exclusion—the disease is not Leucocythemia; there is not, I think, any organic change in the liver; there is no malignant affection of any visceral organ; there is no albuminuria; there is no tubercle.

"There is very irregular excretion of urine, the specific gravity of urine varying from 1.010 to 1.036, but there is no increase of quantity.

"I have examined her for piles, and there are two external ones *quite passive* and within the rectum, within reach of the finger; there are three nodulated masses with broad bases, one very large and exquisitely sensitive to the touch. I presume these are hæmorrhoidal; they want the softness and granular character to be malignant disease of the epithelial type, and they are not hard enough for schirrus.

"Over the whole cardiac region, extending in

the line of the aortic arch, and extending in the line of the aorta along the spinal column, there is an arterial murmur synchronous with the systole—the murmur is too diffused to be localised as proceeding from the thickening of the mitral valve, and as there is a fair second sound (certainly a *little* clouded) and no regurgitation there can scarcely be any great difficulty at the semilunar valves. We have therefore one of two conditions: the murmur is either purely anæmic (from previous loss of blood), or it is from a diffuse aneurismal dilatation of the aorta. On the whole I think it is anæmic as you have already diagnosed. The probability of its being aneurismal should not however be forgotten: there is sometimes a sort of relative dilatation which amounts almost to aneurism, where in persons of large organic build the volume of blood has been greatly reduced. Mrs. F—'s case may partake of that character.

“As to treatment. I have had injected into the rectum once daily tincture of iodine with Gallic acid; medicinally I have ordered an oxygenated chalybeate water, as follows:—peroxide of hydrogen one drachm, sulphate of iron one grain, sulphate of potassa ten grains, water four ounces, with a little chloride of sodium and sugar. This she takes with comfort and takes it as though it were an article of diet.

“In the way of food I have mapped out a rather plentiful roll, beginning with a little good

Burgundy for breakfast, with plenty of animal food in the day in the minced or semi-fluid form and with fresh fruit."

In the course of about a week I received another letter from Dr. Richardson in which he informed me that our patient was continuing the treatment, and apparently with some advantage. The supposed internal hæmorrhoids were less sensitive, and she appeared to be gaining a little strength but that a recent examination of some blood had shewn the presence of a considerable number of white corpuscles, a symptom not present at his first examination; he expressed himself as feeling a little anxious on the subject, and promised to let me hear from him again.

Some five or six days after this, I had another communication from him, in which he spoke of Mrs. F— not improving as rapidly as he could wish, but that still he thought she appeared better on the whole.

The blood, he said, which he had examined that morning, as far as he could judge, did not contain too many white corpuscles, but he undertook to make another examination, and proposed to do this the day before Mrs. F— returned to Stafford, which she purposed doing in the course of a few days or a week at the furthest; he also suggested that I should make a microscopical investigation of the fluid as soon after Mrs. F— returned as possible.

On the same day that Mrs. F— got back I received a letter from Dr. Richardson informing me that he had seen our patient just before she left, and considered her as being improved by the treatment she had been submitted to, but at the same time admitted the progress not to have been such as he expected, and that, as at his last examination, he had again found a great number of white blood cells in the blood viewed, he looked upon the case as being a very critical one, and that there were great grounds for being anxious as to the result.

The day after Mrs. F—'s return I called upon her and obtained some blood, which upon placing under the microscope I found to contain only about the natural number of white blood cells, they were large, and as near as I could count one to a hundred red cells—but upon this occasion I was also struck by the unusual number of "free nuclei."

I thought Mrs. F— looking better than before she went to town, but still very ill; she could move about better than she did and suffered less distress from any little exertion; her pulse was 70, and certainly both fuller and firmer; she remained in Stafford ten days or a fortnight, and then returned into Yorkshire.

I wrote at once to Dr. Richardson telling him that I had been unable to find any unnatural number of white corpuscles, and he wrote me a

reply informing me that the blood he had last examined had been submitted to the view of a very experienced microscopist, as well as to his own, and that there could be no doubt about the enormous excess of white corpuscles, and he further said that it was not the only case in which he had known the white cells to be present in excess at one time and not at another—such a case of *alternation* had not, however, before presented itself to my observation.

After Mrs. F—'s return to Yorkshire she kept gradually improving, the treatment was persevered with until the month of March in the present year, when finding herself perfectly recovered and able to take exercise freely, even to the extent of a walk of six or eight miles, she abandoned it, and in consequence of her being on a visit with her Staffordshire friends I have this day—June the 20th, 1866—had the opportunity of seeing her, and it gives me pleasure to say that she seems in exceedingly good health.

She is, I think, scarcely so stout, but the pallid, worn, cachetic look is all gone, and the lips, gums, and conjunctivæ have all regained their proper healthy colour. She says that she feels as well as ever she remembers to have done for many years; sometimes, but very rarely, she has seen a little blood in the water closet, and if by any chance her bowels get constipated, which they scarcely ever do, she has a trifling amount of pain in the rectum,

with these two most infrequent exceptions she is not aware that she has a single thing the matter with her; her blood now only contains the white cells in proper numbers.

COMMENTS.

It is very far from being easy to trace the connection between the complaint this lady most certainly suffered from, and the appearance and disappearance of the Leucocythemic condition of her blood.

That mere loss of blood caused the appearance of an excessive number of white cells is, I think, a most unlikely occurrence, and supposing for one moment that it did produce such an effect, it certainly could not at the same time account for their disappearance, and yet, as far as one could judge, this cause, *i.e.*, loss of blood, was the only one which occasioned and maintained all her unfavourable and really serious symptoms.

One could of course fall back upon the expression very commonly used to account for some unaccountable state or condition of the system, namely, "*some error of nutrition*;" but I do not see that this would throw much light on the matter, although it would inevitably and most naturally lead to the question—*what error?* Before an answer sufficiently definite to be satisfactory could be given to this enquiry, the whole physiology

of the blood would have to be discussed, and the various and conflicting theories with regard to the uses of its numerous constituents to be reconciled.

The several writers who have contributed to the literature of this subject—"Leucocythemia"—do little more than refer to the excessive number of white cells present in this complaint, and point out the frequency of their too great number in connection with disease of the liver, the spleen, or the lymphatics without accounting, or at any rate successfully accounting, for such an association.

This indeed need not excite surprise; for any explanatory remarks, having for their object the elucidation of this matter, would necessarily include a life history of lymph and chyle cells, as well as of the red and white cells of the blood; and thus the whole region of what I may, perhaps, be permitted to call fundamental physiology, would be introduced, and this too (in our present limited state of knowledge) without establishing any conclusive fact, but, on the contrary, leading to an endless controversy as to "causes" and "effects" about which we are, as yet, in no position to justify our forming, much less giving, an opinion.

It may be something—indeed it is something—to be able to surmise and to argue on the probability of the "hæmatin" and "globulin" of the red corpuscles serving the purpose of replacing the fibrin and albumen of the "liquor sanguinis," and thus discharging the function of preparing the

nutrient material of nerve and muscle ; and it must, by a parity of reasoning, also be something to be able to argue on the same grounds that the white cells subserve a similar purpose for the other albuminous tissues.

Yet, unless it could be PROVED that the white cells are an intermediate stage between chyle or lymph cells and the red corpuscles of the blood, and that a leucocythemic condition of the blood consists in an arrestation of the conversion of the former into the latter, I cannot see that any practical deduction of an useful kind could be arrived at, the more especially as in such cases of the complaint as have come under my notice the blood has not seemed to be deficient in the number of red corpuscles, although these bodies have been to all appearance not well charged with hæmatin, but the affection has appeared to be marked by a superabundance of white corpuscles, and this certainly was the case with my patient Mrs. F—; no deficiency in the number of red cells was observed, but the white cells were *at times* far in excess of their ordinary and natural number.

That diseases of the liver, the spleen, or the lymphatics—but more especially of the two last—should exert an influence detrimental to the development of those bodies generally thought to be so necessary to the preparation of a pabulum for nervous and muscular tissues, one can, by the adoption of existing theories, very readily under-

stand, but in the case I am commenting on these viscera gave no evidence of their being in any way implicated (unless any one would be bold enough to assert that there can be no hæmorrhoids without the existence of portal congestion) and the only detectable ailment the lady could be proved to have was internal hæmorrhoids, these tumours being in a position but little likely to interfere with the accomplishment of a perfect nutrition.

Mere loss of blood is, as I have before said, by no means sufficient, or, one would rather say, at all likely, to account for such a superabundance of white cells, for in point of fact these should have drained off pretty equally with the red, and we have therefore no other conclusion to draw than that it was not a case of anæmia only, but of faulty blood formation occurring at intervals, and from causes so obscure that they baffle any attempt at defining them.

It is scarcely necessary to say that the treatment had two objects in view—the stoppage of the loss of blood from the piles, and the supply of the material believed to be required for the formation of the hæmato-globuline of the red cells; and as the case ended in a very complete recovery, I suppose the treatment is properly entitled to at least some—nay a very fair share of any credit attached to such a termination.

HISTORY XI.

LEUCOCYTHEMIA.

Of all the pathological changes present in disease there is one of such paramount importance—one that has in all time attracted so much attention, and yet which is even now so imperfectly understood, that if I could be tempted to forget my own ignorance and presume to point out in what direction investigation should be, and could be, most properly directed for the purpose of discovering the cause of disease and the means of preventing or removing it, I should unhesitatingly indicate “The Pathology of the Blood.”

It is not intended by this to speak disparagingly or slightly of the labours of Hewson, of Gulliver, of Wharton Jones, of Paget, of Müller, of Wagner, of Nasse, and very many others, all of whom are entitled to receive, and it is satisfactory to know that they have received, the highest credit for what they have done, but simply to express a belief that there still remains an undiscovered mine of knowledge, which future investigators may

open out by continued researches into what would at one time have been called, I suppose, "Humoral Pathology."

The thousand-and-one circumstances that may be—that absolutely are—in daily operation to interfere with an integrity of nutrition essential to the attainment and maintenance of perfect health, must tend to vitiate that fluid which contains the elements of food and of poison—life and death.

That an examination of matter so complicated in its constitution, containing elements so opposite and capable of producing such contrary effects, should have yielded to those interrogating it answers conveying so much and yet so little information, need not and of course does not excite surprise, but there are good grounds for entertaining the conviction that with the possession of such facts as have at present been acquired, and by means of the gradual but certain increase of scientific knowledge which may fairly be looked forward to, a further and more important insight into the nature of blood, both in health and disease will be arrived at, and as a necessary consequence, the treatment of our various and varied disorders will be rendered alike more simple and more certain.

The grosser aberrations which are recognised as occurring in the blood during disease (I wish to be understood as meaning by this those alterations which present a physical difference of appearance admitting of being recognised by the unaided eye,

or with the assistance of the microscope), are, perhaps, already tolerably well known, but it is quite plain that there must be conditions of an antecedent nature which lead, as it were, to these seen physical states—stages in which the exercise of chemical force, or forces, must have played a prominent part, but which our present knowledge of animal chemistry has not admitted of our either discovering or understanding.

The imperative necessity which exists for a perfect comprehension of that which may, without impropriety, be termed the *philosophy of food* to enable us to contribute something towards that great end—a perfect nutrition—is now so generally acknowledged by the profession as to render any remarks on the influence produced by it on the blood altogether superfluous; but we are, as yet, scarcely alive to, scarcely sufficiently acquainted with, the important part played by the respiratory act in perfecting the preparation of nutritive material for its ultimate purposes, and especially as regards that conversion of carbon into carbonic acid, believed to be, but not *proved* to be, the origin of our animal heat and at the same time of converting our venous into arterial blood, and of producing most probably other changes in this fluid *of which we are now altogether ignorant*.

I have already said that it need not, and does not, excite surprise that so complicated a fluid as the blood should have been investigated, in all its

conditions, with so little apparent result of a practically satisfactory nature; but it does seem strange that such a palpably plain and easily-to-be-detected state as that of "Leucocythemia" should have been altogether unnoticed, or, if noticed, unreferred to until twenty years back, when it was first recognised by Professor Bennett.

This unnatural condition of the blood, however, although characteristic and usually associated with the most serious and fatal alterations of organic structure is still, as far as its origin is concerned, unaccounted for.

The state and necessary conditions of organs—the functions in fault, and in fact all the necessary defects for the production of this disease are hidden and, so far, unknown; we may have cases in which the liver, the spleen, and lymphatics are suffering from the most severe and destructive structural changes, and yet no excess of the white corpuscles shall be present, and yet again other instances will occur presenting symptoms indicative of the same functional defects, at least, which will be attended by an extreme excess of white corpuscles.

In plain language then, the cause of leucocythemia is not understood, but its frequent appearance during the progress of certain diseases of the liver, the spleen, and the lymphatics irresistibly leads to the inference that it results *in some way* from the imperfect performance of the *function* of one or more of these organs, but it is far from

being probable that these organs must, or should be, altered or diseased in their ultimate structure to produce excess of white cells.

It is not often that the opportunity is afforded of noticing the first appearance of a leucocythemic condition of the blood; in the majority of cases this state has been in existence, in all probability, for some time before the patients come under the observation of the physician, and in others in which its presence might reasonably be anticipated it never appears at all.

The following case which was under my care in the Infirmary at Stafford affords, however, an instance in which the morbid condition of blood was traced from its commencement.

CASE.

A— B—, a shoemaker by trade and sixty-one years old, was admitted on November the 3rd, 1865; he was a short and lightly framed man, his chest possessing that peculiar depression of the sternum which constitutes the well known and characteristic form called the "Shoemaker's Chest;" his complexion was described as being usually "fair," but when I first saw him it might have been more properly called "a yellowish white;" the conjunctivæ were slightly tinged with bile; his tongue was moist and white, and his breath offensive; his breathing was hurried, and this was made much worse by the slightest exertion; his

pulse was weak and thin, and when he was in the recumbent position, 86 in the minute; his urine was very dark in colour but deposited no sediment, and had a specific gravity of 1.020—its chemical reaction was acid.

He said that his bowels acted every day, but that the evacuations were costive, their colour varying, sometimes being "a mahogany brown" and at other times "as white as chalk."

He described himself as having been out of health for at least two years, but not to have been so ill as to be obliged to abandon altogether working at his trade until about six weeks before applying for admission at the Infirmary.

He spoke of his habits as having been the reverse of temperate, but for some months before entering the Infirmary he had felt so much out of health that he could not take the stimulants, nor indeed the food, that he had been accustomed to; he occasionally suffered a good deal, he said, from nausea, and sometimes vomited after having taken a meal. He complained of considerable pain in the epigastric region, and also immediately under the floating ribs on the left side.

On examining the chest by percussion, it afforded natural sounds in every direction excepting over the second, third, and fourth ribs on the left side, here the flattening of the chest seemed to have brought the walls almost in actual and close contact with the heart—the area of dullness being ex-


tended greatly beyond its usual and proper limits, whilst the impulse was not only felt but seen in a manner that I have rarely met with.

Upon applying the stethoscope a moist "rhoncus" was heard in the course of the bronchial tubes, and the vesicular respiration was slightly crepitant in character—the crepitation being most marked at the lower and posterior part of the lungs; although the impulse of the heart could be so plainly seen and felt, both the first and second sounds were a little muffled. With the first sound, and heard plainest at the base of the organ, there was a very clear "blowing murmur;" I could not, however, detect any subclavian murmur.

There was considerable tenderness over the whole hepatic region on the right side, and this tenderness extended quite across to the neighbourhood of the spleen; both liver and spleen were considerably enlarged, the vertical dullness of the former organ being five and a half inches, that of the latter eight inches, whilst the transverse dullness extended to nearly eleven inches.

The abdomen was swollen and tympanitic on percussion without any sense of fluctuation.

He was ordered to take a dose of podophyllin and a mixture containing phosphate and carbonate of soda with spirit of chloric æther in compound infusions of gentian and orange peel in equal parts. His diet to consist of milk and beef tea with three ounces of sherry daily.



Thinking, from the enlarged condition of the spleen and liver, as well as from the anæmic look of my patient, that it was very probable the blood contained an excess of white corpuscles, I had some taken from the finger and placed under the microscope, using a power 250 diameters; the examination, however, did not shew any excess of these bodies—I counted one white corpuscle to 85 red—but a somewhat considerable number of free nuclei were to be seen, and the red corpuscles appeared particularly full and their outline very distinct, as though the fluid in which they floated was of a low specific gravity; their number was quite as great as is usually seen in healthy blood, and they arranged themselves in “rouleaux” very soon after being submitted to an examination.

On my visit the next day I found that the podophyllin had produced three or four large evacuations, firm in point of consistency, very offensive in odour, and nearly white in colour; his urine remained unaltered in any way, and he complained of great nausea; his pulse beat 82 in the minute, and was somewhat fuller and firmer.

I directed a mustard plaster to be applied over the epigastrium, and changed his mixture for one containing small doses of dilute nitric acid in a bitter infusion, and as he expressed a strong dislike to his milk diet I altered it by directing that he should have fish or poultry.

I next saw him on the 7th and found him to

be in most respects just the same as he was on the 4th: his bowels had acted regularly, but the character of the evacuations had altered somewhat for the better, being not quite so pale and their odour less disagreeable; his urine was in colour much as before and still contained bile, its specific gravity being 1.022; his pulse was 80, and its strength and general character rather improved.

He had slept tolerably well, but complained of having uncomfortable dreams and waking in a fright; he also spoke of having a very bitter and unpleasant taste in his mouth, and upon one or two occasions he had thrown up his food.

He had dressed himself and walked about the ward; the exertion, however, produced great shortness of breath, and this obliged him to return to bed again; his legs had also begun to swell. I again examined his chest, but the lung and heart sounds remained unaltered. The treatment was not interfered with.

On the 8th and 9th he continued in much the same condition, but on the 10th he complained of considerable abdominal uneasiness, and he had had slight diarrhoea, the stools being much darker in colour than before; he likewise said that he felt extremely weak and suffered so much from breathlessness on the slightest exertion that he really could not manage to go so far as the water-closet.

As he complained of being very much swollen about the abdomen I examined this part, and there

was evidently some, although not much, amount of fluctuation to be felt, his legs, too, unmistakeably contained fluid in the areolar tissue.

He was certainly getting more anæmic, and I therefore again examined his blood; upon this occasion using the same microscopical power as before—250 diameters. The number of white corpuscles was much greater than it had previously been, but still the excess in their number could not be called relatively very great—there were about ten white corpuscles to a hundred red—the white cells were nearly all of the large kind, and their outline, as well as that of the red cells, was particularly clear and well defined, but the number of cells appeared smaller than on my first examination, the field of view being not nearly so crowded with them.

I thought it right now to alter both the medicine and the diet, substituting preparations of iron for the dilute nitric acid, and directing him to take a liberal meat diet with red wines.

From this date—the 10th—he seemed for about a week to improve a little, at least he was more cheerful, and expressed himself as suffering something less in his breathing; his bowels were rather inclined to be confined, and he scarcely made sufficient water: the colour of the urine was very dark; it still contained bile, and had a specific gravity of 1.028.

On the 13th I once more examined the blood

and was surprised to find how greatly the number of white cells had increased. I counted forty-four of them to a hundred red, they were still almost all of them of the large kind. Upon the application of acetic acid the red cells were destroyed, the white cells lost their granular appearance, the nuclei becoming particularly distinct; in some instances the nucleus seemed to have a central depression *or* to contain a nucleolus, whilst in others the nucleus was double and not unfrequently crescentic in form.

I noticed no particular alteration in any of his symptoms during the next fortnight or three weeks.

He became gradually more anæmic in appearance, and each specimen of his blood that I examined seemed to contain more and more white corpuscles, so that one felt surprised, at length, how such blood could serve the purposes of nutrition, or indeed in any way sustain life.

When perfectly still, his breathing was quiet enough, but as the case progressed the slightest exertion, and a movement of almost any kind became an exertion and threw him into an agony of breathlessness.

By the time he had been four or five weeks in the Infirmary, the ascites was somewhat considerable, and the anasarca condition of his lower limbs was greatly increased. As his bowels were very constipated, and the quantity of urine he

made was very scanty, I felt it to be necessary to make some effort to relieve these two pressing necessities, and ordered him to have broom tea with spirit of nitric æther: these and other diuretics produced no good effect whatever, and consequently, as his inconvenience from dropsical effusions was becoming so great as to be immediately dangerous, I ordered him a cathartic of compound powder of jalap and bitartrate of potash, requesting that he might be watched and supplied with stimulants if necessary, for the state of exhaustion in which he appeared to be was such that I felt considerable anxiety as to what might ensue from the operation of the dose.

Three free evacuations were produced, the two last being very copious and fluid, and although it was necessary to give some stimulant, yet the weakness of the patient did not seem to be so greatly increased during the operation of the medicine as I feared that it would be, whilst the relief afforded to the breathing and other urgent symptoms was of the most marked and satisfactory character.

He also now made water much more freely, and, indeed, his general condition was improved by the course that had been pursued, but the condition of the blood—the state of the liver and spleen—all remained the same, and therefore the relief was necessarily very temporary.

In the course of a very few days all his ascitic

symptoms began to return, and whilst his former relief had tended to put him into a state of almost unnatural cheerfulness, the retrogression in his condition served to throw him into the most extreme mental despondency, and he announced to me that he wished to leave the Infirmary; his friends likewise expressed a very strong desire that he should do so to enable him "*to die at home*," and although I used every argument that I could to induce him to remain, it was all in vain, and he left the Institution on December the 28th, 1865.

He lived about three weeks after going from the Infirmary, and his relatives resisted every attempt and every inducement that was held out to them to allow a *post mortem* to be made.

On the day before he left the Infirmary I made my last inspection of his blood. There appeared to be scarcely any red cells, whilst the field was crowded with white cells, the large ones being still the more numerous, the free nuclei seeming to have diminished in number.

COMMENTS.

However much one may regret that the verification of the diagnosis as to the condition of the liver and spleen in this case could not be demonstrated by an after-death examination of these viscera, I conceive there cannot be the shadow of a doubt as to their diseased condition, and the state



of the blood of course was removed far enough from anything like conjecture by the repeated examinations that were made of it during the time the patient was at the Infirmary.

The remarks made in the previous history—"Alternating Leucocythemia"—would seem to be in many respects so applicable to the present one that for me to make any further comments, either upon the origin or nature of white cell blood, may appear uncalled for; but whilst admitting that any lengthy discussion upon these points would be somewhat out of place in a work intended only as one of clinical history and observation, yet I can scarcely let pass the opportunity of saying a few words having reference to what I believe to be two very different blood affections, but which I have not unfrequently noticed as being confounded by practitioners the one with the other, anæmia and leucocythemia.

To explain what I mean it will be necessary that I should make known what I believe to be, or what I understand to be, meant by the term "Anæmia."

It is to my mind a condition of the blood in which there is no alteration in the natural relative number of red and white cells, but in which the former are defective in their contents, not possessing the proper amount of hæmatin, or colouring matter, the number of such cells, however, not *necessarily* being deficient.

But by the term leucocythemia I recognise an abnormal state of the blood in which the white cells are greatly in excess of their proper number, whilst the red cells are not *usually*, and certainly not *necessarily*, fewer than they would be in a state of health.

There may be, and my experience tells that there are, modifications of these two unnatural kinds of blood.

We may have "Anæmia" according to the above description, and yet in exceptional cases there may seem to be a deficient number of red cells, the white cells appearing in excess, so that in such cases there will be found apparently too few red cells, too little hæmatin in them, and relatively, too many white cells, but still it is really only a case of "Anæmia."

Then, again, we may encounter cases of a truly "Leucocythemic" nature, in which, whilst an enormous excess of white cells is seen, the red cells may be either fewer in number, without their being deficient in hæmatosin, or they may be both less numerous than they ought naturally to be, and also short of their proper colouring principle—hæmatin.

Now, whilst "anæmia" pure, and "anæmia" with its modifications, might possibly occasion some anxiety in the mind of the Physician, "leucocythemia," either pure or modified, should awaken the gravest consideration, because the former will be found most probably to depend on some mere

functional disturbance, whilst the latter will prove to be the result of the most fatal alterations of ultimate structure of visceral organs, the competent performance of whose functions is essential to the maintenance of life.

I have seen innumerable cases of anæmia, both pure and modified, permanently recover, as, of course, every one has done; but I never yet saw a case of persistent leucocythemia, pure or modified, *end* in any other way than in the death of the patient; the first—anæmia—may arise from functional disturbance alone, the latter—leucocythemia—*invariably* depends upon structural change irremediable in its nature.

HISTORY XII.

HYSTERICAL FACIAL PARALYSIS?

Although Professor J. H. Bennett says, at page 435 of his "*Principles and Practice of Medicine*," that "hysteria is always associated with uterine derangement," yet he scarcely can, indeed it is impossible that he can, mean this assertion to be expressive of his ideas as to all those perversions of nerve functions, which are every now and then found to prevail with certain specimens of the male as well as of the female sex; for he says at page 428 of the same work, that "nothing can be more vague than this term"—hysteria.

But notwithstanding the very generally admitted vagueness of the expression, we nevertheless find it commonly made use of by the profession to designate conditions the cause, or causes, of which have baffled all attempts at their discovery. Thus, when any symptoms of nervous disturbance occur which do not admit of being accounted for by some obviously plain reason, it is customary to characterise the phenomena displayed as being of

“hysterical” origin; and of course forthwith these cases are considered to be such as must inevitably be connected with some derangement of the generative organs and their functions; and, as a consequence, the remainder of the first sentence quoted above from Professor Bennett’s work, viz :— “to which the practitioner’s attention should chiefly be directed,” has received the most implicit obedience.

That disorders of the nervous system of almost all kinds, and that some hysterical disorders especially, are amongst the most obscure as to their origin, and the most tiresome and difficult to treat of all the affections with which we are familiar, is a fact that few, I think, will be disposed to dispute; whilst many of the nervous ailments last mentioned possess such a capability of accurate imitation, and, in addition, assume such protean forms, that their diagnosis cannot always be arrived at with the rapidity, precision and accuracy, attainable in many other ailments.

Still there are, fortunately, many landmarks, if one may so call them, that enable us to discriminate between those simply functional nervous disorders of so-called hysterical character, and those serious lesions of nerve structure with their graver consequences, the symptoms attending which the former are apt to imitate; and thus I suppose it happens that we remain satisfied with the use of a term the meaning of which would appear to point

only to a state dependent on uterine disturbance, but the real import of which is not actually so restricted, but extends significantly to a class of nervous affections not usually dangerous, as far as the life or future health of the individuals afflicted may be concerned.

A ready method is thus afforded by which we ourselves recognise the safety, and by which we can communicate to others our belief in the safety, of our patients, simply by the use of this undoubtedly vague term "hysteria," although the symptoms of the cases may appear to the uninitiated to be of a frightfully alarming nature.

There is, however, a very great evil that may result, and that very probably has often resulted, from the application of this term hysteria to such cases of perverted nerve functions as do not allow of being put down positively to some evident and well made out causes.

It is in my opinion tolerably certain that this expression has been, and is, used not unfrequently to cover our own ignorance, and this too sometimes, although perhaps not often, in cases where serious neural changes of hidden origin are mistaken for, and treated as, the evanescent effects of a mere hysterical condition.

The case, a history of which I am about to give, affords, I conceive, an instance in which the general experience and opinion of the profession is not altogether in accordance with my own.

The freaks of hysteria in producing a variety of paralytical conditions have afforded matter for comment to almost every writer upon practical medicine, and they one and all are careful to point out the distinguishing marks by which the *true* and the *false* forms of the malady may be recognised.

Amongst those spurious forms of paralysis which are believed to be solely and wholly the effect of a functionally hysterical state, "facial paralysis" is, as if by a common consent, excluded.

But whilst admitting that high authorities in practical medicine make no mention of hysteria being one of the causes capable of producing facial palsy, I am constrained to dissent from the sweeping assertion that local facial paralysis is never the result of a purely hysterical state; and the commencement, course, and termination of the following case, I think it will be allowed, justifies me in taking exception to a very generally received, and acted upon, dogma.

CASE.

A— H—, an engine driver, aged 31, was admitted at the Stafford County Infirmary on December the 30th, 1864.

He was an extremely good-looking man, with a florid complexion, light coloured, short, thick, curly hair, large blue eyes, and a set of good white teeth. He was but little less than six feet high,

and of a large and well-proportioned frame, with a good muscular development, but not *fat*.

But for one circumstance—facial paralysis—his general appearance might be said to have been indicative of vigorous health. He had been twice married, his first wife having died from puerperal fever about eighteen months before his admission to the Infirmary—his second marriage took place about four months previously to my seeing him—he had two children by his first wife, and his second wife was believed to be pregnant at the time his ailment commenced. He had been attacked about a fortnight prior to coming to the Infirmary with facial paralysis and some pain in the head.

From the account I received it appeared that after his first wife's death he had given way to habits of intemperance; and being, according to his own account, of a very amorous nature he had also indulged in what might be termed an indiscriminate sexual intercourse—he had fortunately for himself never contracted either syphilis or gonorrhœa. His occupation was that of an engine-driver on the London and North-Western line, and he had up to the time before mentioned taken his "turn" with great regularity for several years.

He said that for some weeks before his illness he had experienced at times a most exuberant flow of spirits, whilst at other times he was so depressed that he not only felt disposed to cry, but actually did do so, and this without his knowing why or

wherefore. On the night of his attack he had performed his usual journey in his customary manner, but as the weather was bitterly cold he had had several "drams" on the road. On leaving his engine to go home he adjourned to a public house and had "a little more drink;" during the time he was in this house he had one or two fits of uncontrollable laughter, and subsequently a quarrel with a "mate," which ended in his having a fit of crying; a reconciliation took place and this was followed by another fit of immoderate laughter, and during this he was led home.

He retired to bed immediately he got home, feeling, he said, in his usual health; he slept well as far as he remembered, but on waking in the morning his face felt very strange; on rubbing it with his hand he found that something was wrong, and on getting out of bed and looking in the glass he perceived that it was, to use his own words, "all on one side." He said that he was very much alarmed at the time, and then began to feel a pain in the front of his head: he did not remember to have had any sensation of pain *before* he looked in the glass.

A medical man was sent for, and he had been under treatment up to the time of his coming under my care, but although the distortion of his face was represented as not being so great at the time of his admission as it had been, nevertheless it was very considerable, and he still complained of

pain in the front of the head and just over the eyes.

Having already spoken of the man's general appearance, and given such an account of his previous history and state as I was able to obtain, I shall now proceed to notice his symptoms and condition when I first saw him.

He was in bed, and immediately on my coming to the side of the bed he sat upright, and commenced talking in a rather hurried and excited way, describing the manner of his attack, what had been done for him, and his present feelings; and during this time he kept continually putting his hand up sometimes to his head, but more frequently to the face.

It was the left side of the face that was paralysed, the muscles of the right side being drawn up, giving to it a shortened appearance, and an expression between a smile and a laugh, not being exactly either the one or the other; the eye-lids on this side were semi-closed, and the mouth pulled upwards and to the right—this portion of the disfigurement was not, however, very great.

The left side of the face had a lengthened, blank, dolorous look, the muscles appearing to be stretched and the skin having a tightened appearance, the eye on this side was not only wide open but the lower lid seemed drawn down, and this more particularly at the outer angle.

He spoke with tolerable distinctness, and indeed

plainly when he gave a sort of support to the left side of his face, and I found it was this circumstance that occasioned him to so repeatedly put up his hand to this part. The pupillary openings of both eyes acted readily enough to the influence of light, but whilst he could close the right eye-lids easily he could not shut the left lids at all.

He heard plainly and well with both ears, and the sense of feeling on both sides of the face seemed perfect. He swallowed his food without the least difficulty, but during mastication he was troubled in a trifling manner with an escape of saliva; this, he said, had been a great annoyance at first, but was now much better. His pulse was full and soft, and beat 82 in the minute; his bowels were very constipated, in fact he complained that they were so obstinate that he had considerable difficulty in getting them to act at all. He made a good deal of pale-coloured urine, free from sediment, with a slightly acid re-action, and having a specific gravity of 1.019; he did not void much water at a time, but in small quantities and very often—it showed no evidence of containing albumen, neither could sugar be detected in it.

His tongue was moist and whitish in colour, the back part of it being foul, whilst all the papillæ—filiform, fungiform and circumvallate—were remarkably large.

He had a particularly well-developed chest, and the respiratory and heart sounds were perfectly

healthy, the rhythm of the latter being well maintained—in short, all his visceral organs seemed in a healthy condition as far as could be ascertained by an external examination. The transverse and descending portions of the colon were very much loaded. His spine was carefully examined, but there was no detectable tenderness of any portion of it, nor indeed anything to lead to the belief that he suffered from spinal irritation of any sort. The volitional movements of all his limbs were perfect, and tactile sensibility in no way interfered with. He had a good appetite—almost too good—and he slept well. He informed me that he was a good deal troubled with “erections,” amounting almost to priapism, but without seminal emissions.

My examination gave me no reason to conclude that there was any organic mischief, whilst the history given by the patient pointed, I thought, to a state analagous at least to the form of hysterical paralysis seen occasionally in females, and I determined to treat the man as if he laboured under this affection, although I knew that a general impression prevailed with medical authorities that *local* facial paralysis was not a form of palsy ever hysterically imitated.

I directed that an enema consisting of castor oil, turpentine, and thin gruel, should be administered immediately; two hours afterwards a dose of croton oil to be given, and five grains of bromide of potassium in distilled water three times a day; he

was also ordered a milk diet with a pint and a-half of beef tea daily.

On seeing him the following day I thought the distortion of the face was scarcely so great, although others who saw him said they did not perceive any appreciable difference. The enema had brought away some immense masses of hardened fæces, and soon after he took the croton oil he had two very large evacuations of a semi-fluid character, gingerbread-brown in colour, and possessing only the ordinary and natural fæcal odour.

His appetite remained good; his tongue was cleaner, but all the papillæ had the same enlarged appearance I before made mention of; his pulse continued the same; he wanted to make water less frequently, and he had not been so much troubled with "erections."

I had requested the nurse to notice the appearance of his face when asleep, and she told me that not only had she done so, but some of the patients in the same ward had observed it as well, and they unhesitatingly assured me that it assumed pretty nearly a natural look—they could not say that both sides were precisely alike, but the difference between the two sides bore no resemblance to that observed when he was awake, for "the eye that he could not shut when awake was perfectly closed when asleep."

He still complained of pain in the front of his head, but this did not deter me from ordering him

to have a cold shower bath the following morning. His spirits seemed to be very good although his fellow patients said that at times he shed tears. His melancholy however was very short lived.

I did not see him the following day, but on my visit the day afterwards I found that the facial paralysis had all vanished. I learned that he emerged from his shower bath, gasping for breath, but no longer "a double-faced man." The nurse said that several times in the course of the day he had twitching of the face "as if it were going to be paralysed again," and even when I was talking to him he seemed every now and then as if he were endeavouring to screw his face into the paralysed form, but the efforts were altogether abortive.

The pain in the head he persisted in saying was still there, but admitted, although with some unwillingness, that it was not nearly so bad as it had been.

I ordered him to have another shower bath and changed his diet, giving mutton or beef, but allowing no stimulants. His bowels were still very constipated. To obviate this I directed that he should take castor oil every alternate morning, the bromide of potassium to be continued.

He seemed for some few days very much altered in his manner. At first he was a good deal excited and very talkative, but he became after the removal of his facial disfigurement very silent and shy.

This gradually wore off, and at the expiration of three weeks from his admission he left the Infirmary perfectly well and without the least trace of his illness.

This might fairly be considered as being the conclusion of the case, but such is not altogether the fact.

He resumed his usual occupation of engine driving within a few days of the time he left the Infirmary, and I neither saw nor heard anything whatever of him until the 8th of November in the following year, when early in the morning his wife came to my house and begged that I would go with her to see her husband, who she thought had had a fit something like he had before, his face being "all on one side," and in addition, he could not speak, however much he tried. I went with her, and there, in bed, lay my former patient with his former paralysed face. I spoke to him, but only to obtain the most terrible grimaces without the least sound of voice.

I gathered from his wife that soon after he resumed his occupation he commenced drinking again, and that for ten days or a fortnight before fetching me he had been exceedingly excited and queer in his manner, being at one time in the most boisterous spirits, and at another just as depressed and melancholy.

He returned home on the night before this last attack "a little the worse for drink," and awoke

his wife early in the morning by making a strange noise and pointing to his face and mouth.

There was no sign of facial anæsthesia, and he undoubtedly heard and understood all that was said: this was rendered evident by his making a hideous attempt at a smile or a laugh when I spoke to his wife cheerily as to the probability of his recovery, and by his affecting a most comical attempt at crying when I spoke in terms of regret at his having relapsed into his former intemperate habits; he also rotated and nodded his head as signifying assent or dissent to any remarks addressed directly to him.

His wife said she could not speak positively as to the then state of his bowels, but she knew that they were usually so obstinately confined that, to use her own language, "he was always taking anti-bilious pills;" his tongue was tolerably clean at the front and edges, but the back part of it was "furred," and of a yellow colour; the urine which he had made a short time before my arrival was pale and clear, it was free from albumen, and had a specific gravity of 1.018.

His pulse beat 90 in the minute, and was full and soft; the pupillary openings acted as they should do, and altogether I could not detect any evidence of intra-cranial mischief.

Upon examining the abdomen I found the same loaded state of the transverse and descending portions of the colon that I noticed upon the first

occasion of my seeing him. During the whole of my examination he eyed me and listened to my observations and remarks with a nervous, uneasy, almost anxious, kind of look and manner. He gave no sort of intimation that he had any pain in the head.

As upon the first occasion, so upon the present, I directed that he should have an enema of castor oil, turpentine, and gruel immediately, and a dose of croton oil two hours afterwards, whilst his diet was to be strictly a milk one with a pint and a half of beef tea.

I saw him again the following day—the enema and croton oil had produced their desired effects, and he had passed a comfortable night. The paralysis of the face was in no way improved (it was upon this occasion as upon the last the left side that was affected), but his wife told me that he had managed to say “no” to some enquiry that she had put to him; I, however, could not succeed in getting him to speak if he were able to do so.

The diet was recommended to be continued, and I ordered him to have a mixture containing the bromide of potassium, as before.

Two days afterwards I paid another visit to him—he was up and sitting in the kitchen. The facial palsy was certainly but little, if any, altered; he tried to speak and succeeded in saying “yes” and “no” in a kind of “breathing whisper,” his bowels had acted fairly, he had slept well, and his pulse beat 86 in the minute.

I wished him to have a shower-bath, but the difficulty of getting one was considerable, and his wife was almost reluctant to make any effort to obtain one. I felt persuaded that it would be (as it had before proved) a considerable assistance in the treatment, and I therefore suggested that he should again go to the Infirmary: the proposition, however, was received with no favour, and he said rather loudly and very sharply "no."

I then, as plainly as I could, pointed out to his wife the probable *immediate* benefit that would result from his having a shower-bath, and as he so resolutely resisted the proposal for his going to the Infirmary, I began to instruct her how she might extemporize one with the aid of a colander, when he suddenly and violently shouted out "I'll have none of that," and rushed up-stairs.

At the moment I do not know which of the two was most surprised, his wife or myself, at this instantaneous recovery of his voice; we followed him up-stairs, and he was walking up and down his room declaring that he would not be "soused with cold water in that shameful manner."

I observed that the face was still presenting its paralysed appearance, and he continually put up his hand to the affected side of it. It seemed to me to be undesirable to allude to the sudden recovery of his voice: I therefore merely addressed a few cheerful remarks to him and then left.

I allowed two days to pass before calling on him

again, and then on my visit was glad to find that his face was in a natural condition. He had not had the shower-bath, but his wife apprised me that when he awoke in the morning after my last visit "his face was quite right." I did not see him again, but heard at several subsequent periods that he remained well, and was living a more temperate life than he had done for some time past.

COMMENTS.

The vague nature of the term hysteria has been remarked upon at the commencement of this history, and having regard to the etymology of the word it seems simply ridiculous to apply it in any way to the male sex; but that there are cases in which men suffer from those perverted emotional and sensational feelings usually recognised as hysterical when occurring in women, is a generally admitted if not an established fact.

The man whose case I have been relating was most probably by nature a very excitable person, and his habits of intemperance—I mean by this expression to include his sexual propensities—tended to disturb that due performance of nervous function which in persons of a more equable temperament it is almost impossible to unhinge, and to some extent the character of his vocation might have tended to produce a similar influence; whilst still further the whole of these circumstances might

have been aggravated by his being unable at times to obey the calls of nature as far as his bowels were concerned: indeed he himself attributed the constipated state of his body principally to this last difficulty.

Upon first seeing this man I must confess that I had some doubts as to the cause of his condition, and it was only on obtaining every particle of information I could get as to his antecedents, and a close and careful consideration of each symptom, that I arrived at the conclusion that his entire affection resulted altogether and alone from perverted nerve function.


I have, as others have, over and over again in practice found the hysterical condition in women to be connected with, if not absolutely dependent upon, a loaded rectum and colon, I consequently addressed my first remedies to the removal of any source of irritation that might be in existence in this neighbourhood, and certainly on both occasions my patient A— H— was benefited by the proceeding.

Being also of the opinion expressed by Sir Thomas Watson, in his admirable and elegantly written *Lectures on the Principles and Practice of Medicine*, vol. I., page 710—"that there is more virtue in cold water in hysterical diseases than in any other single remedy," I did not fail to make use of it in the first attack, and, as will be remembered, with the best possible effect—indeed, so

good was it that its *remembrance* on the second seemed sufficient to produce the desired result.

I do not think it requisite to refer to all those symptoms which, by exclusion, themselves led me to consider this case and to treat it as one of hysteria, but I cannot help observing that I believe there is no writer on clinical or practical medicine who names hysteria as *one* of the causes *capable* of producing this kind of partial paralysis, or, as it is most commonly called "local palsy;" and yet it is difficult to understand how an affection capable of apeing any other form of paralysis should have a limit set to its powers of imitation by this one.


Whatever may be the opinion of others, to my mind the foregoing history is demonstrative that facial paralysis cannot fairly be excluded from the rôle of characters within the compass of hysterical mimicry.



HISTORY XIII.
ON CARDIAC APNŒA,
FORMERLY CALLED ANGINA PECTORIS.

The old nosological term "Angina Pectoris" has recently been changed by one of our modern English medical Classics into "Cardiac Apnœa." The new term in a pathological point of view and in a physiological point of view is, it seems to me, most to the purpose; breathlessness commencing at the heart is in short, as the term implies, the meaning of the disorder when we refine our language of disease so as to make it clear and accurate. But the old term notwithstanding was good—cardiac apnœa is, unmistakably, "heart pang," and no *external* symptom could be more forcibly expressed than in these two words "Angina Pectoris."

There is no wonder that the old men named it as they did; they stood outside the arcana; they saw none but external evidences of wrong: to them the conception of a combination of air and blood in the lungs was not born; to them



the necessity of a correct balance of force between the circulatory and respiratory forces was unseen, and to them the necessity for a perfect balance of action between the right and left sides of the heart was equally unknown. They were thus driven to speak by their light, and so speaking they expressed what they saw truly "breast pang."

Perchance in a future day some man will speak even of the author of "Cardiac Apnœa" himself as outside the arcana, and seeing something nearer to the first cause of the malady under consideration than a break between air and blood, will re-name the affection from his point of view and write of his past as we write of ours.

Under whatever name known, under whatever system of pathology classified, "Angina Pectoris," or to speak more correctly Cardiac Apnœa, has been recognised in and from very early times; indeed, if its study has ever been neglected it has been most neglected in modern times. Our modern medical class books define it indifferently compared with those of the preceding, and but for one or two of our essayists we should virtually have lost sight of a finely and correctly rendered history of the malady as it appears to the practised eye. For this reason, and in order to strengthen the labours of those who have preceded me, and to bring out in strong relief the truth of the *old* not less than the *new* physic, I relate the following case in which with its striking and characteristic

features was combined this almost novelty, that I was enabled to follow it from its commencement to its termination.

CASE.

A— H—, a private patient, first consulted me in the early part of the year 1862, for some flying rheumatic pains about the joints of his arms and legs. He was a person in comfortable, not affluent, circumstances, and sometimes travelled for a house of business, but much more frequently passed his time at the bar first of one hotel and then another, drinking freely of malt liquor and getting “fuddled” without being in a state of incapable intoxication.

He was married, and forty-seven years old, good-looking, with thin light hair, dark blue eyes, and a somewhat but not very pale complexion. His height was about five feet nine or ten inches, and in person he was stout, indeed too stout, with a strong disposition to corpulency. In manners he was lively not to say vivacious, and being tolerably well connected he appeared never to miss the opportunity when under the influence of beer, to “talk of his friends.”

Upon his first visit to me, his principal complaint was of the rheumatic pains he occasionally had in his knees and his shoulders; his tongue was white and foul at the back part of it, and his breath

disagreeable; his pulse beat 76 in the minute, soft and weak, with an occasional intermission in the beats.

His bowels, he said, acted regularly, indeed if anything, too often, and the evacuations were never hard; they were of a pale colour generally, but sometimes very dark; he had never suffered from piles.

His urine was passed freely, it was pale in colour, and its specific gravity was 1.018, it had a strongly acid reaction, and upon standing, deposited a light-looking sediment; it was not clear upon being first made. Upon heating it the first effect of the heat was to render it clear, but as soon as it commenced boiling it deposited a copious white sediment, which was re-dissolved with some slight effervescence upon the addition of nitric acid.

Upon examining his chest, which was so clothed with flesh, and the mammæ so large that it almost resembled a woman's, I could not, upon percussion over the pulmonary region, find anything abnormal, but the extent of dullness over the præcordia was considerably greater than is natural.

Respiration was performed twenty-two times in the minute, and each part of the chest examined with the stethoscope only afforded the proper lung sounds, although these were rather weak in character.

Both the first and second sounds of the heart could be heard, but not with great distinctness;

they, like the lung sounds, seemed weak—there was not, however, the least murmur—the rhythm of the heart's action was not perfect; after the second sound there would sometimes be a longer pause than at others, and at the apex of the organ there was a peculiar trembling which I have frequently noticed as being indicative of thin walls. With so much natural covering as his chest possessed, the examination was not altogether what I could have wished it to have been.

He said he had a good appetite, eating animal food two or three times a day, and that he enjoyed a fair amount of exercise, which, however, I took the opportunity of saying his appearance did not indicate. He admitted that he was addicted to "beer."

I prescribed for him guiacum mixture night and morning, with two doses of disulphate of quinine during the day. I also recommended that he should substitute small quantities of brandy and cold water, or dry sherry and cold water, for his beer, and advised his diminishing his quantity of animal food, and increasing the amount of exercise he took.

A fortnight after this interview he called again upon me, and expressed himself as feeling in every way better. He was certainly looking better, and this principally in consequence of being much thinner; he spoke with considerable complacency about his diminished circumference, and applauded


his own resolution in having acted so rigidly up to my directions.

He had, he said, no rheumatic pains, except now and then, in his left shoulder joint; when this did occur, he represented it as being very severe, and extending from the shoulder down the arm in the direction of the biceps. His tongue was much cleaner, and his pulse somewhat fuller and stronger. I did not examine him any further, as he seemed to consider himself as being "quite well."

I neither saw nor heard anything more of him until the 16th of August in the same year, when I was summoned to see him at his own residence, the messenger informing me that he was "ill in bed with rheumatic fever."

On entering his room, I was very much struck with his altered appearance. He was so much stouter than when I last saw him that, independently of his being in bed and undressed, a condition in which I had never seen him before, I could scarcely recognise him. His face was nearly, to my thinking, twice its former size, and at this time it had a feverish look, and was bathed in perspiration.


He said that he was in some considerable amount of pain at the time he sent for me, but was easier just at the moment of my arrival. His knees and elbows were much swollen, the movement of the joints did not cause one half the amount of pain that I had seen persons endure



when suffering from the same disorder. His tongue was moist and white, and the pulse 86, soft and weak. The urine was rather high-coloured, and had a specific gravity of 1.023; there was no evidence of its containing albumen, but there was a slight deposit of uric acid. His bowels had not been moved for two days. He was thirsty and had no appetite.

It appeared that he had been in bed for four or five days, and was unwilling to send for me, because he had been leading a very irregular and intemperate life. His knees and elbows were, he said, swollen some days before he began to feel any pain in them, or to feel at all ill in himself, and he had hoped that it would all pass off without medical assistance; but what he most wished to see me for was a pain in his left side, "*just at the heart,*" and he had a great dread that something would happen to him, as several times he had felt very short of breath.

He was perspiring very freely, I therefore just removed the clothes sufficiently to allow of the introduction of the stethoscope, and then listened to the action of the heart. I immediately heard a "to-and-fro" sound, the instrument being applied over the apex of the organ: it was also heard with considerable distinctness over the whole site of the heart, but I failed to detect any endocardial murmur; the first and second sounds were to be heard more plainly than I should have thought



they would have been from the thick covering of fat which clothed the walls of the chest. The rhythm of the heart was still irregular, but I did not notice the peculiar trembling as upon my first examination.

I prescribed for him a dose of calomel and colocynth, and four grains of iodide of potassium in distilled water every four hours, likewise two grains of disulphate of quinine twice a-day. I also directed a blister to be applied over the region of the heart, and on its removal a large warm bran poultice to be placed over the blistered surface; his diet to be new milk *ad libitum*, with a pint of beef tea and four ounces of sherry in the twenty-four hours.

The following day he was easier from pain, and the shortness of breath of which he complained had not occurred again. I could not make use of the stethoscope in a way to be altogether what one could have desired in consequence of the blistered surface, but still I could detect the "to-and-fro" sound, although I thought it not so evident as it was the day before. He had had some comfortable sleep. The cathartic had acted freely, the evacuations being very bilious and offensive; his tongue was not so white, and he said that he felt a little more appetite; the pulse was 80, full and rather stronger. He had made water freely, it was darker in colour and deposited urates in large quantities, the specific gravity was 1.023.

I did not alter either the medicine or diet, and on my next visit, which I made on the 19th, he was decidedly better; the swelling of his joints had diminished and the pain in them was much less; he had slept very fairly, and expressed a wish to be allowed to have some solid animal food. I could hear nothing of the "to-and-fro" sound; his tongue was getting to look much better—less white and feverish—the urine still continued to deposit urates, and his bowels acted regularly; the pulse was 76, full and tolerably soft, but still at times intermittent; he had ceased to perspire, and seemed altogether to be progressing favourably.

I did not think it desirable to alter his medicine, but allowed him to have poultry or fish instead of the beef tea, and gave him permission to sit up the following day *provided* he continued as well as we had reason to expect he would be. I left directions that if he showed any symptoms of being worse that I was to be communicated with, but in the absence of such communication I should not see him again for three or four days.

On the 23rd I again saw him; he was sitting down-stairs, and declared that he felt quite well except from weakness, and that it was from this alone that he suffered; he was so weak he said that going up-stairs "took away all his breath," and he was obliged to sit down before he got to his bed-room.

I once more examined the condition of his

heart, but could detect no "bruit" of any sort; the action of the organ was, as it had always been since I first saw him, weak and the rhythm irregular, but beyond this I could not perceive anything unnatural; his pulse was 74 in the minute, soft, and I thought both fuller and stronger than I had ever felt it.

He was looking quite as well as he could be expected to do after such an attack, and taking his previous course of life into consideration; but still there was a somewhat anæmic look about him, and on this account I prescribed a small quantity of iron with the quinine he was taking. I ordered him to clothe in flannel, to use no undue or sudden exertion, and to let me know how he was going on in the course of a few days.

At the expiration of a week he called upon me, and he seemed very much improved in health and appearance, but he said his joints were very stiff at times, and some friends had suggested his going to Buxton if I saw no objection to his doing so. I, of course, did not, and he stated his intention of leaving for that place the day but one after his visit to me.

I neither saw him nor heard from him for more than a month, and then accidentally met him in the street. He said that he was about to call upon me to tell me how well he felt, and how much he had benefited by his sojourn at Buxton; but he added, "I sometimes have those sensations of want

of breath, they don't take me often, nor indeed at all, unless I have been doing anything in a hurry; when, however, they do come on I think they are worse than they were." I requested him to accompany me to my house, and I once more examined his chest.

I could detect nothing whatever beyond the condition I have before mentioned as being present at my first examination. He was considerably thinner than when seized with his acute rheumatic symptoms, but looked infinitely more healthy than I had ever seen him, and he himself said that but for the attacks of breathlessness, which sometimes seized him, he felt better than he remembered to have done for years.

Not being able to discover any symptom whatever of valvular affection, I could only come to the conclusion that his seizures depended upon a large, dilated, weak-walled heart, and I ordered him to take a liberal diet with a moderate amount of stimulants, at the same time cautioning him against any sudden or over exertion, and to avoid, as far as possible, all unusual excitement.

Some few days after this I was sent for in great haste to see him, the message being that "they thought he was dying." I was fortunately at home and reached his residence with all possible speed. He was sitting on a sofa with a chair in front of him, the back of it being towards him and his hands firmly clenched on the top part of it; he

was gasping and struggling as if for breath, the mouth open, the eyes staring, the head thrown far back, and the general expression of the countenance being one of extreme agony, whilst the limbs seemed fixed with almost tetanic violence.

My first movement was to feel for his pulse, which was beating with irregularity but great rapidity; from his immoveable position I could not get at his chest, and I knew it was useless to put any question to him as he evidently could not speak. I asked for and obtained immediately some brandy and hot water, and with great difficulty got a fair portion of it down his throat; within a few seconds of his having swallowed it his condition improved, his attempts at breathing became less violent, his hands relaxed their grasp of the chair back, and he sank on the sofa with an exclamation of "thank God!" I now got to his chest and applied my ear to it—I had no stethoscope with me—the action of the heart was tumultuous and irregular, so much so, indeed, that I could not make out *distinctly* the two sounds of the organ.

By degrees he recovered so as to be able to converse, and he said that this was quite different to any attack he had had before, and he fervently hoped he might not have anything resembling it again, at the same time beseeching me to do all I could to prevent it. I learned that he had wished to go from home by a particular train and had been hurrying to accomplish it, and when just

on the point of leaving the house he was seized with a violent darting pain which he said "shot through his chest and into his limbs, and seemed to effectually debar every attempt he made to take breath."

I remained with him for more than an hour, and when I left him he really appeared so much better that it was difficult to realize the fact of his having so recently had such a seizure. The frightful appearance presented by a person under such circumstances is sufficiently alarming to prevent any one who has witnessed it having any other than a vivid recollection of it, but the *apparent* complete recovery which may take place soon after the attack makes scarcely a less lasting impression.

I promised to see my patient the following day, and did so, at the same time making once more a most complete and careful examination of his chest, but only, however, to have my former examinations and the conclusions I drew from them corroborated; there was no difference whatever, except that the trembling which I have so frequently spoken of was more distinct if anything.

He was cheerful but expressed some anxiety about himself, and begged me to say or rather tell him whether I considered that his condition was a dangerous one: I gave him a modified opinion, but put his friends in full possession of the facts; they had heard of angina pectoris and knew its danger. I told them what I considered to be the condition

of his heart and spoke of the *possibility* of there being some fibrinous deposit, but in the absence of being able to assign any absolute reason for such a thing beyond his rather recent rheumatic illness, I reserved giving any *definite* opinion upon this point.

He continued in much the same state of health as he had been for some time, and had no other attack of a like nature for many weeks; he then, however, had another early in the morning after leaving the water-closet, but it was by no means so severe as the first one, and although bad and terrible enough, it altogether passed off in a much more easy manner.

I had directed what should be done in the case of his being seized in the same way on any future occasion, and he subsequently had two or three fits of the same general character; he came out of these with so much less suffering than he did on the first occasion that I really believed both he and his friends began to think either that I had alarmed them unnecessarily, or that stimulants would at least be sure to ward off every danger.

However, on the 17th of March in the following year I was sent for in the night to him—he had been suffering from diarrhoea during the day and in the night was obliged to go to the water-closet, and on his return to his bed-room was seized as upon the first occasion. I found him in bed with the clothes huddled round him and his hands

tightly gripping some of them ; he was half reclining at the moment I entered, having had some stimulant given to him. He made an attempt to address me and instantly gave a scream, struggled and gasped for breath—the sight defies description—he made one terrible effort to get breath and then sank in bed—there was not a single beat of the pulse or of the heart—he was dead.

POST MORTEM.

The *post mortem* was made the day after his death—there was strong “*rigor mortis*”—the body was that of a well-nourished person and corpulent. On opening the chest the lungs were seen lying in their natural position, they had no appearance of inflammation or congestion, but looked of rather a paler grey than these organs are usually seen ; there were no pleuritic adhesions ; the pericardium was adherent to the heart at one or two points, and the internal surface rougher than it ought to have been, and on opening it a small quantity, perhaps half an ounce, of fluid escaped.

The heart was pale in colour and much larger than the organ is normally found to be : its base was covered with fat, and the coronary vessels stood out in a very distended state ; upon laying open the left ventricle the walls were found to be not thinner than is natural, and this was also the case with the auricle. The ventricle was empty, but there was

a clot of dark-coloured coagulated blood in the auricle; the walls of the right auricle were so thin that it seemed almost as if a slight thrust with the finger would have burst through them; lying within the cavity of the auricle was a clot of clear fibrine, adherent to but not organised with the endocardium, from this clot a fine filamentous prolongation of it passed into the right ventricle, in which it appeared about the size of a large earth-worm. It was twisted, as it were, upon itself, and passed through the valves of the pulmonary artery into that vessel to the extent of about an inch and a half, terminating in a fine point; it did not appear to have formed any attachment to the "chordæ tendiniæ," or to either the tricuspid or pulmonary valves. The "twists" that I have spoken of were just at the valves, and I thought the appearance might probably be caused by the effect of the current of blood at these spots. The walls of the right ventricle were quite as thin as those of the auricle, and all the cavities were much enlarged.

A microscopical examination of the muscular structure did not show anything like fatty degeneration.

All the other organs were in a healthy state except the liver, which was enlarged and its vessels very much congested.

I did not open the head, as I was requested not to do so unless it appeared absolutely necessary for discovering the cause of death, and as there had

never been at any time a single symptom indicating mischief within the cranium, I considered that nothing could be gained by the proceeding.

COMMENTS.

The case thus recorded is as clear, as perfect, an illustration of true Cardiac Apnoea as any I have ever read. Some of the cases of "Angina Pectoris" mentioned by William Butter in his treatises upon the disease, published in 1796, offer, perhaps, certain sharper outlines and more desperate tetanic symptoms, but they are not on the whole more definite; while taking it from its beginning to its termination it is a more typical case than any of those given by Dr. Richardson in his sixth essay of the first volume of the "Asclepiad," on Cardiac Apnoea. The case has important bearings in two or three directions, to which I would invite attention.

In the first place, we see the symptoms travelling by progressive steps from bad to worse, each such progressive step marked, there is every reason to believe, by a special pathological change, and all indicating that the painful symptoms were in proportion to the increasing feebleness and embarrassment of the heart. Thus we had, in the first place, the symptoms pure and simple of a feeble heart from structural change, painful symptoms but enduring; next we had a febrile attack with indications of pericardial adhesion, and thereupon an

increase of severity in the spasmodic seizure; a step later and there is deposit of fibrine in the heart itself, and with that the extremity of embarrassment and the fatal attack.

The influence of the arrest of the circulatory current on the other muscles, especially the involuntary muscles, was striking. Darwin in his "Zoonomia" treats of angina pectoris as due to two causes, either of which may be primary, viz.—spasm of the heart, or spasm of the diaphragm: he says,—

"It is possible that either a fixed spasm of the diaphragm, or of the heart, which are both furnished with weak antagonists, may occasion sudden death; and these may constitute two distinct diseases."

In my case there could be no doubt that the first modification of the normal life was in the heart, but on this the diaphragm quickly followed and the chest became fixed. But I name the theory started by Darwin, because there may be cases of disease in which, in truth, the primary mischief is situated in the diaphragm. The diaphragm is, as stated in the paragraph quoted above, a muscle having a weak antagonist; it is subject to change of structure like the heart itself, the blood vessels by which it is fed are subject to arteromatous change, and yet further, the nervous supply by which it is called into continuous action is derived from the same source as the nervous

supply of the heart. In brief, except in the matter of shape no two muscles are more allied than the heart and diaphragm, and it is certain that the suspension of action in either of them would lead to a development of symptoms nearly the same. If the theory be true, if there are really two distinct forms of angina pectoris, each one presenting analagous symptoms, but each having a different seat—cardiac or diaphragmatic—we have in the circumstance much difficulty cleared away. We may account at once for those extreme cases of the malady in which, as it has been stated, no disease of the heart or of its coronary vessels has been found after death, and we are directed in all such cases to turn to the muscular diaphragm in search for lesions there in its muscular structure, in its tendinous expansions, or in its vascular channels.

There is a sound point of interest in this case, in the effect produced on the muscles of voluntary and semi-voluntary action by the sudden withdrawal of the blood. They were thrown, one and all, more or less into a state closely resembling that of tetanus. The same symptom has been observed before. Butter describes, that in one of his cases so powerful was the muscular contraction that the bladder was involuntarily emptied of urine. The elder Dr. Latham (J. Latham) in the thirty-first volume of the *Medical and Physical Journal*, records an instance in which so paralysed were the interruptions of the pulse during the

paroxysm, that the direct effect of the withdrawal and emptying of blood in the muscles of the abdomen could actually be seen. "The pulsations," he says, "might be considered as so many heavy undulations, affording, as it were, a spasm in space as well as in time between each, and during which interval the muscles of the abdomen were so visibly affected as to remain in a state of tremulous or vibratory motion, which gradually diminished until the next wave or swell was succeeded by another similar vibration."

Dr. Richardson gives a case in which the symptoms were wonderfully rapid, even during the first seizure, and so essentially spasmodic in character that death was induced within a few minutes: in that case the heart was found firmly contracted on itself, the cavities being so empty of blood that they looked as though they had been washed out.

No one can read the account left us by Cabanis of the sufferings and death of Mirabeau, without recognising in their extremest degree the signs of breathlessness commencing at the heart, accompanied, in his case, with severe contraction of the diaphragm and pain in the abdominal viscera. It is probable that in Mirabeau's instance there was not only lymphatic deposit over the whole of the surface of the heart, but attachment to the diaphragm and disease of that muscle.

In viewing the symptoms which mark the progress of cardiac apnœa during the paroxysm,

one cannot but be conscious of the light we, as pathologists, have obtained from the labours of those amongst us who have tried to read disease by physiological induction. When Stannius and Brown-Sequard brought on stiffening rigor of the muscles of the limbs of living animals by cutting off the supply of arterial blood, and re-produced flaccidity by allowing the blood to return, they demonstrated what occurs in some cases of angina pectoris when, during the paroxysm, the heart failing to supply the essential of muscular power—blood—there is instant spasmodic rigor remitting only when the stream of blood is once more allowed to return. When again Richardson produced rigor in the healthy muscles of a limb of an animal by subjecting it to the three-inch spark of Ruhmkorff's induction coil, overwhelming, that is to say, the muscles by the introduction of electrical force, he shewed us the simple truth, so plainly observed in extreme cases of angina pectoris, that whenever the propulsive power of the heart is enfeebled it may be spasmodically affected by the most trifling external irritation, or by an excess of force passed to it by and through the nervous circuit.

There is also another lesson taught us by these experiments, viz.—that we should be cautious in reference to the diagnosis we form in some particular cases of sudden death. No one who has seen cardiac apnoea in its most severe form, *and during a paroxysm*, can have failed to observe how

nearly the paroxysm approaches in appearance the true tetanic convulsion. In the case I saw and have related, it would have required (had the case been seen for the first time when the last severe and fatal attack occurred) extremely good capabilities of practical observation to distinguish it from a case of sudden idiopathic tetanus, or tetanus produced by poison: even the characteristic scream was not wanting. A physician who was well up in the subject of cardiac apnœa, who had seen the patient before, who was on the look-out for the event, and who was cool and collected while making his observation, would make, I have no doubt, a correct diagnosis as between cardiac apnœa and tetanic spasm, but a person not so well informed—not so *aut fait*—in the whole matter, would readily enough fail, and might afterwards have tortured out of him such symptoms, as would fit any convenient medical hypothesis suitable or requisite to fill up and render complete the necessary gaps of a legal subtlety based on circumstantial evidence.

In studying cases of the kind I have just given, there is another point, very distinct in itself, but which should always be kept in mind as bearing on the exciting cause of the paroxysm. We have abundant proof that in the production of an attack of cardiac apnœa there are always, we may say, two factors at work—there is a previously prepared condition of the heart, if I may be permitted such an expression, and there is excitation of the nervous

supply or force. But for this, life would be impossible under the circumstances in which angina pectoris happens; as it is the patient may go on for days, weeks, nay months without a paroxysm: he will be unwell, feeble, perhaps nervous and excitable during this period, but free from the acute attack; at last there is some sudden action on the nervous system which tells upon the heart, and then the symptoms appear in all their terrible vehemency.

This nervous exaltation, this communication of excess of force from the nervous system to the heart, may have its origin in many parts, so that when once the primitive cardiac lesion is present it becomes extremely difficult to prognosticate whence the excitement of the paroxysm will come from. It may be a mental shock, telling directly on the organ; it may be irritation communicated to the terminal filaments of the pneumogastric; it may be excitability in the centres of the organic nerves; it may be excitement direct from the cord, or in some way dependent on the circulation of improper blood; or it may be irritation from the external surface of the body or some of the outer mucous passages.

I may go even further in the way of illustration. When once the heart is disposed to irregularity of action on excessive nerve stimulus, the excitant may be so refined as to be scarcely detectable by the senses: impressions may be made on the retina, on

the olfactory expanse, or on the ear, as shall be scarcely known or felt as being unpleasant, and shall yet be all sufficient to disturb the feeble resistance of the embarrassed heart.

In the case given above, I have recorded a pure instance of cardiac apnœa, in which the symptoms terminated in a characteristic paroxysm. It is not, however, necessary that every marked case should run precisely the same course. The case I am now about to relate is an illustration of cardiac apnœa, following upon rheumatic fever and fibrinous deposit in the heart, with removal of the deposit into the general circulation and installation thereupon of a new class of symptoms altogether.

CASE.

W— H—, a miner by occupation, married, and 47 years of age, was admitted as an in-patient at the Stafford Infirmary on January the 12th, 1866.

He was a man of rather large frame, and about five feet ten inches in height: his eyes and hair were dark, and his complexion sallow, and altogether he had the appearance of possessing a bilious temperament. His general development was muscular without being fat, limbs long and powerful, and I could not help thinking him to be, as far as his outward configuration was concerned, a remarkably good specimen of his class.

From all I could learn, his habits must have

been particularly sober, very unlike the general character obtained by "miners." He was a dissenter, and had been always particularly attentive to his religious duties.

His health had been, he said, tolerably good until about eighteen months previously, when he suffered from a severe attack of rheumatic fever, which lasted for nearly six weeks, and it was almost three months from the commencement of his illness before he was able to resume his work. After the rheumatic fever he never felt so well as he had previously done, and he was troubled a good deal with indigestion. He also complained of having suffered much at times from attacks of difficulty of breathing, but he never had any cough, and he could scarcely remember to have ever taken cold.

The symptoms that he complained of principally when admitted to the Infirmary were—flatulence after meals, accompanied by a sense of oppression in his breathing; his bowels also were obstinately constipated, and his sleep was uncomfortable, being disturbed by frightful dreams.

His pulse beat very irregularly, and 82 in the minute; the character of the pulse was weak and small; the tongue was whitish with a swollen, sodden look, and marked at the side by the teeth.

His urine had a specific gravity of 1.010, it was pale in colour and had an acid reaction, boiling and treating with nitric afforded no evidence of the presence of albumen; when allowed to stand there

was no sediment beyond a slight cloud of flocculent looking material. Examined microscopically nothing could be seen beyond a few epithelial cells. He had no appearance of any dropsical effusion.

On looking at the chest there was nothing unnatural in its appearance, it was both wide and deep, in fact a capacious well-formed chest; there were marks of leech-bites, and over the cardiac region evidence of a blister having been applied. Percussion over the pulmonary surface gave no indication of any unnatural condition of the lungs, but the area of dullness over the site of the heart extended beyond its proper limits.

A careful stethoscopic examination afforded no evidence of any lung mischief, but the rhythm of the heart's action was materially interfered with; every now and again there was a pause after the systole of very uncertain duration, whilst over the base of the organ, and continued along the course of the arch of the aorta was a slight breathing, rather than a blowing murmur, accompanied by a peculiar harshness; it was not quite a grating sound but nearly amounted to it; this seemed at times to be absent and then to present itself again; at the apex this murmur was very indistinct, but there was a singular kind of tremor which could actually be perceived through the walls of the chest, and this appearance was evident even at the epigastrium. I have in the previous case referred to the presence of a similar symptom.

He had been under medical treatment for some time before he came to the Infirmary, and said that as far as his attacks of difficulty of breathing were concerned, he was so much better that he rarely experienced any discomfort except after a meal, when the distention from flatulency was occasionally so great that he scarcely knew what to do with himself, and besides this, he felt so weak that he really could not follow his occupation. He suffered a good deal from heart-burn.

I attributed the abnormal heart-sounds to the effects of injury sustained most probably during his illness from rheumatic fever, and determined to direct my treatment principally to the correction of the gastric irritability under which he appeared to labour.

I directed a blister to be applied, and ordered him a dose of podophyllin every other night, a mixture containing phosphate and carbonate of soda with chloric ether and tincture of ginger, in equal parts of infusion of orange peel and compound infusion of gentian. His diet I wished to have consisted principally of milk and beef-tea, but he expressed so great a repugnance to the milk that I altered it for mutton in small quantities; I also ordered him to have three ounces of brandy a day.

Under this treatment his dyspeptic symptoms gradually improved, and he took his meals with an appetite, feeling but little and sometimes no incon-

venience after having eaten them; he was also more cheerful, and whilst on first coming into the Infirmary, and for several days afterwards, he preferred to remain in bed, he now got up and dressed himself, and expressed a wish to be permitted to sit in the day ward.

Although the podophyllin produced the desired effect, the bowels showed no disposition to act naturally and without the aid of medicine; the evacuations that he had, as the result of the podophyllin, were always very light in colour.

His urine was continuing pale in colour, free from sediment and with a very low specific gravity, being now only 1.008; it was still slightly acid in its re-action, and I could detect no evidence of albumen in it; the quantity made was moderately large, indeed quite sufficient.

I again examined his chest, but both the pulmonary and cardiac sounds remained unaltered in every way, and his pulse, although four beats in the minute less (seventy eight) than when he came into the Institution, still retained its irregular and weak character.

On the eighth day from his admission he appeared to be still improving as far as his indigestion was concerned, but he thought he had taken a cold (a thing he said he could hardly remember ever to have done before), as he had a short, dry, tickling cough, but he remarked, "for all that I am getting well fast, and I hope you will be able to discharge me next Friday."

On my visit to the Infirmary the following day, I was told that W— H— was not so well, and that he was in bed, having had “a kind of fit in the night.” I went at once to him, and got the following account of the occurrence. He said that he felt that his bowels were about to be acted upon, having awoke from a most uncomfortable dream with this sensation: he put on his trousers in a great hurry, and was hastening to the water-closet, when, about the middle of the ward, he was seized with a sensation of breathlessness and fainting, whilst almost at the same moment he had a terrific pain in his left groin, thigh, and leg, and it was from the effect of this that he fell down.

He declared most positively that he never was for a moment insensible, and that he was sure that it was no fit that he had had.

I examined his leg and thigh carefully, but there was nothing to be perceived beyond a little fullness of the superficial veins of the leg—there was some considerable tenderness on making pressure over the groin, but no impediment to the free movement of the limb in every direction.

At the time of his fall some of the patients in the ward left their beds and assisted him back to his bed, but I could not get any information from them, nor from the nurse, that would lead me to believe that he really had had a fit of any kind, and the house-surgeon, who got to him as soon as he could, said that if it really were a fit of any descrip-

tion, it must have only been a fainting one, for beyond great pallor of his countenance, he could perceive nothing to lead him to think he had suffered from a fit of any sort.

I desired him to remain in bed, and ordered the limb to be fomented with warm water. He requested to be allowed a little more stimulant, and as there was nothing in his general condition, nor in the condition of his pulse, to contra-indicate it, I acceded to his wish.

I saw him again the next day, and found him to be in some respects much in the same state as the day previously, except that he pronounced the limb to be much less stiff than it was the day before; the superficial veins were certainly quite as prominent as when I last saw them, and I thought the temperature of the limb felt lower than that of its fellow, but he persisted in assuring me that it was altogether better, and voluntarily moved it about in a variety of directions to show me that it really was so. I felt his pulse as usual, and although its intermittent character was in no way different, it was nevertheless weaker than I had before felt it, and seemed to tremble under the finger. On applying the stethoscope to his chest, I was surprised to find that the peculiar grating kind of murmur which I have before mentioned was much less distinct than I had ever heard it: it was still there, however, and the want of rhythm in the heart's action was just the same. I now

for the first time suspected that there might have been some detachment of a fibrinous concretion, and that the leg symptoms might have arisen from a temporary arrestation of the circulation in the limb, but yet the symptoms were not sufficiently grave in their character to lead one to fear any immediate danger, and certainly the condition of the leg had become in no way worse in the course of forty-eight hours.

I directed the whole treatment to be continued, and added moderately large doses of carbonate of ammonia to the mixture he had been already taking.

Feeling anxious about my patient, I visited him early the following morning. I was told almost as soon as I entered the Infirmary that W— H— had had another fit and was speechless. When I entered the ward he was sitting up in bed supported by a bed-rest: his face was particularly pallid and he looked anxious, at the same time he pointed first to his mouth and then to his leg.

He was perfectly conscious, and there was not the least distortion of any part of his countenance. When asked to put out his tongue he did it immediately, the organ being protruded in a direct line, and not inclining more to one side of the mouth than to the other. The pupillary openings of the eyes were more widely dilated than I had before noticed them to be, and they acted to the influence of light, but in a very sluggish manner. The vessels of the conjunctivæ were much injected.

His pulse continued to beat irregularly, and had the same trembling feeling under the finger that I noticed at my last interview with him. It was very weak, and seventy in the minute. Upon examining his leg the difference that had taken place in its appearance since the day before was most marked; from the knee downwards, including the foot, it was of a dark purple colour, in patches, and the superficial veins stood out like, and felt as hard as, cords. The limb was cold, and no pulsation could be detected in the arteries of the foot, leg, and thigh.

When the leg was handled or moved it seemed to occasion him considerable pain, and he made several attempts to speak for the purpose of giving an explanation as regards certain circumstances, but his efforts at articulation were altogether ineffectual. He had made water and his bowels had been acted on; of both these operations he was quite conscious and intimated by signs to the nurse what he required.

I was very desirous of obtaining all the information that could be got as to the manner of this second attack, but he could afford me no assistance in the matter. Some of the patients said that "he got out of bed apparently to make water, and that he made a strange noise and fell down." The nurse was then summoned and she found him, she informed me, in bed and speechless—the other patients had helped him into bed; she also re-

marked that he appeared quite conscious and sensible when she got to him, but "he was very pale and seemed to be out of breath."

I once more examined stethoscopically the condition of his heart, and upon this occasion could hear no murmur of any kind whatever: there still remained the irregular action, the faulty rhythm of the organ, but neither with the systole nor diastole was there anything unnatural; the first and second sounds were heard with *tolerable* distinctness, but unaccompanied by even the slightest detectable murmur. The peculiar tremor which I spoke of as being both heard and seen at the apex of the heart, and during the impulse especially, continued just as before.

From the evident want of power exhibited by his pulse, and the remote possibility of being able to produce solubility of any fibrinous matter interfering with the general current of the blood, I further increased the amount of ammonia in his medicine.

He took his food without difficulty, but also without any relish; indeed, it was only as the result of persuasion that he could be induced to take nutriment of any kind. I requested that he might be supplied with anything he wished for, and recommended his taking an additional quantity of alcoholic stimulant. The affected leg I ordered to be kept warm with cotton wool and flannels.

I now could not feel in my own mind any

doubt as to the nature of the mischief that had taken place, and I was at the same time well aware that anything in the way of treatment would be of little avail. I nevertheless would have continued, but on the following day his friends removed him to his home, where he died three days afterwards under a continuance of the symptoms of obstructed circulation.

THOUGHTS ON THE TREATMENT OF CARDIAC APNŒA.

Surely there are no cases so difficult to treat, so perplexing, so anxious, as those of Cardiac Apnœa. Still even in the worst cases there are certain rational lines of treatment which may be tried as being likely to alleviate, and, in this sense, to remedy the existing evils. The treatment has in it two distinct elements tallying with the two factors of the malady.

The first element is to relieve, as far as possible, the existing organic mischief and the consequent embarrassment.

The second element is to prevent the nervous excitations which call forth the paroxysm.

In cases where the organic change is simply dependent on excess of living, luxury and sensuality, there cannot be a doubt as to the relief obtainable by a careful regimen and a mildly reducing course of medicines.

If in such cases the patient be not advanced in life, and he can be restrained in the use of alcohol, potatoes, sugar, pastry and bread, and if, with restrictions on these particular articles of food, he will take sufficient and at the same time quiet bodily exercise, there can be no indiscretion on our part, if we prognosticate in a favourable manner as to the ultimate result of the treatment. The case I have given at length at the commencement of this history is a good example of what can be effected under this system, for my patient improved under it, to my satisfaction and almost surprise, so long as he abided by it.

With this diet, medicines of a gently reducing power are useful, or, it would be better to say, medical means of such kind are useful. I believe that sweating by the Turkish bath has proved of service, but in the country we have no means of putting it into practice. Fortunately we have as good if not a better remedy in our purgative podophyllin. This most useful medicine is as efficient as mercury—some say more efficient than mercury—in its influence on the hepatic secretion—it clears the whole of the alimentary canal; and it neither distresses nor depresses when properly prescribed. It combines with tonics such as quinine, it combines with the anodyne extracts, and it combines well with Dover's powder or an antimonial.

In cases where there was much plethora, our forefathers had a strong liking for setons or issues

for the cure of "angina." Darwin, who was a good, and certainly a most unprejudiced observer, relates four cases, all of which were relieved by issues either in the arm or thigh. This derivative method has now well nigh been forgotten, but there is trustworthy evidence that it has been of service, and there is reason also on its side. A seton would possibly do good in two ways: it would tend to reduce excess of body, and possibly it might reduce, as a blister does, general nervous excitement. I had not learned at the time when my first case came before me that setons and issues had been so much relied upon in a past day, or I should certainly have given them that fair trial which I now reserve for any other case of the kind that may come before me.

In instances where the symptoms either follow or become much worse after an attack of rheumatic fever, and where it may be supposed that lymph has been thrown out upon the pericardial surface, it is a reasonable treatment to carry on the administration of iodide of potassium until a decided effect has been produced on the body, and to sustain that effect for a time in a mild degree. In the case I have related, there was evidence of improvement under this method, and of a very decisive character.

Presuming in any case there was a clear diagnosis that fibrinous deposit had occurred in the cavities of the heart, it would be reasonable to carry out a determinate alkaline treatment, keeping the

blood thoroughly under the influence of the alkaline solvent, but short of any detrimental influence on the corpuscles. For this purpose no mixture, perhaps, answers so well as one consisting of five grains of the citrate of ammonia and iron, with ten grains of citrate of ammonia added—the whole to be dissolved in one or two ounces of water, and taken three times in the day.

But the most important point of practice after all is to keep in view the second factor of the paroxysm—the nervous excitation which calls forth the attack, and to keep that in subjection. Here so many influences, each one requiring to be conciliated are at work, that all the watchfulness of the most acute physician can hardly suffice, while over some which he cannot foresee, and which spring out of the ordinary vicissitudes of life, he can exert no control whatever.

Nevertheless there are certain precautions which may be taken, and on which he should strongly enforce attention, and amongst these I notice as first in importance the control of passion; secondly, worry of business; thirdly, excess of exercise suddenly called for, such as running to catch a train, or hurrying up an ascent; fourthly, sudden change in the position of the body, as from the erect to the recumbent position; fifthly, over muscular fatigue; sixthly, venereal excitement; seventhly, exposure to extremes of sound, such as orchestral music, military file firing and the like; eighthly, exposure

to emotional influences, such as tragedy, or even extreme comedy; and, ninthly, to severe and sudden alternations of heat and cold.

In addition to precautions of this nature, the personal conditions should be carefully attended to. The body should be scrupulously clothed to meet the season, so as to ensure an equable temperature: in ablution the water at all seasons should be about the mean of the body, 96° — 100° , and it should never be applied in sudden dashes or showers, as in the shower-bath.

In the way of food much care should be taken, as I have already shown. Every meal, whatever it may consist of, should be most moderate in amount, for death after a heavy meal is one of the commonest phases of angina pectoris. At the same time it is equally bad policy to allow the stomach to become exhausted from absolute want of food. The patient should never even go supperless to bed, but should have a light meal containing animal food easy of digestion: milk, lightly boiled eggs, tripe, oysters, or minced white fish, are all allowable as foods before rest, when taken in moderation. Depressing fluids, especially tea, should be at all times avoided, and all articles which in the experience of the patient are slow of digestion, or cause heart-burn and eructations, or lead to constipation and to white or clay-coloured evacuations.

Alcohol, although very objectionable as a continued luxury in cases of angina pectoris, can, I

think, never be actually dispensed with as an article in the treatment. There can be no doubt that a glass of hot brandy and water or gin and water will often ward off a paroxysm, while during the paroxysm, if the patient can swallow, nothing brings speedier relief to the spasm than the same remedy. I have no doubt the heat plays an important part, but not the sole part, in effecting this. Patients with angina may also partake, in moderation, of wines which are free from acidity and, as the common phrase runs, are "dry."

While these matters of diet and mode of life are under consideration, while any general treatment is being carried out, the very strictest attention must be paid to the digestive functions. If any excess of acidity be present it must be neutralised by alkaline solutions of carbonate of soda, and it is desirable that there should be a free, but not an excessive action of the bowels; these are two points of practice which, though they may seem to be of minor importance, are really secondary to none.

In the main then the treatment of Cardiac Apnoea consists in attention to many details, and I am convinced that by such attention the agony of the disease may often be held off, and the final attack, the fatal catastrophe, averted for many years. In fact cases are not altogether wanting in which with an increase of years, and with the general debility incident to such increase, the

paroxysms have subsided, or become chronic in their character, and the full term of life has been secured. In the example of John Hunter, who fell a victim to the malady, we see a man who, if he could have maintained the *suaviter in modo* as thoroughly as the *fortiter in re*, might have lived, in all probability, for many years to the advantage of science and humanity.

It remains only for me to dwell for a moment on the best means for keeping off or meeting the acute paroxysm. For this purpose a stimulant is always useful, as I have already intimated, and the application externally of a hot sinapism over the regions of the heart and diaphragm is often of essential service.

In the way of medicine a narcotic combined with ammonia perhaps answers the purpose better than anything, and the following is the best combination I know :—Liquid acetate of ammonia two drachms, carbonate of ammonia three to five grains, sedative solution of opium ten to twenty minims, with one or two ounces of water. Patients supplied with this medicine soon learn to have confidence in its utility; it generally relieves pain, relaxes spasm, and has the effect of inducing free elimination from the skin, which is always, or nearly always, attended with considerable relief.

I would that it were possible to write more—to speak of *cure* as well as of *relief*—but ere this can be done we must wait for more light, not for

medical science alone but for all science. In a word, we must know more about the physics of animal motion, and then, *perchance*, we might possibly even see a way to the cure of spasm of the heart itself.

FINIS.



London, New Burlington Street,
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